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ANNUAL SUBSCRIPTION RATES

THE PSYCHOLOGICAL REVIEW

SOME FOLLIES OF 'EMANCIPATED' PSYCHOLOGY [1]

BY H. M. JOHNSON [2]

It has been said that after Fechner had invented the methods of psychophysics, and Wundt had founded a laboratory, and numerous psychologists had learned to manipulate instruments and to calculate, and their leaders had repudiated the method of *a priori* analysis in favor of controlled observation, psychology became 'completely emancipated from philosophy, and assumed her place among the natural sciences.'

It often happens that for a considerable time after his emancipation, the libertine tends to exult in his liberty, and to assert the right to practice rather more of it than he has yet learned to use. The newly-emancipated psychologist has presented such a behavior-pattern. In adopting a new procedure, he had to rationalize his actions before he could proceed freely. Of course, he overspoke himself. He proclaimed that experimentation, even in an undeveloped science, was not a supplement to analysis, but a substitute. It had become unscientific and undignified to work with one's head as soon as it became possible to work with one's hands. It was quite proper to rush into experimentation without first asking whether the question to be answered stated a genuine problem, to which *any* experiment could yield a valid answer. As for logic—it was out of date. To violate its rules, and even to profess ignorance of them, only helped prove that he was a scientist—by proving that he was not a philosopher.

[1] From the address of the retiring president of the Southern Society for Philosophy and Psychology, delivered at New Orleans, December 30, 1931.

[2] Head of Simmons Investigation of Sleep, Mellon Institute; Visiting Professor of Psychology, Graduate School, American University.

20

It is these excesses which I have called the follies of emancipated psychology, but I shall consider only a few of them in detail.

It is Aristotelian logic which provides the standards of proof that hold in universal science and in commonsense practice. In these fields of discourse, no inference is valid unless it satisfies all the postulates and operational rules of that system. Among the procedures which Aristotle lists as fallacies is the one that is technically called 'equivocation.' It is committed whenever two distinct concepts are treated as identical if they bear the same name. Whenever it is employed, two links in the chain of reasoning are left uncoupled, because they have no term in common. The conclusion which is apparently drawn is not a true conclusion, but is unrelated to the premises. Although the *symbols* of the two concepts may be identical to eye and ear, the concepts themselves may be defined by two different sets of properties, or by two different sets of operations. If so, they must not be interchanged in the same discussion. Whenever we limit our examination to the common name, we run the risk of committing this fallacy, which some authors believe to be the most prolific fallacy of all.

It was in the hope of reducing this risk that the 'barbarous' terminology of science was invented. But even that was not enough. A technical word need not be precise, any more than a 'dumb' girl needs to be beautiful. The word is precise if it denotes a single, precisely defined, concept—*i.e.*, one that is defined by a unique set of operations or of properties. Many of our longest and most formidable words are not precise. The very word 'equivocation' has a moral, as well as technical meaning. It denotes a procedure which is more deceptive than direct lying can be, and which is therefore more reprehensible than direct lying in scientific discussion. After the psychologists became emancipated from the discipline of formal logic, they began, most unfortunately, to overlook, then to tolerate, and finally to practice this procedure overtly.

A very large part of the doctrines of modern psychology

has been inferred from observational data by means of equivocation. From time to time various critics have protested that this was so, but they were as one that crieth in the wilderness, where there is none to hear or care. The procedure is being employed, openly and freely, in many authoritative works on psychology; great experimental 'projects,' costing hundreds of thousands of dollars, have been founded upon it; and the organized profession has recently permitted the publication, under its semi-official approval, of certain radio-lectures in which equivocation is featured. These lectures are addressed primarily to readers who have had no special training; they are written in such clear and simple language that anybody can detect the method; and they represent to the layman that psychologists officially consider psychology to be a system that is grounded in equivocation.

As distasteful as these facts may be to those who believe in the value of rigorous thinking, I think we must recognize them, and acquaint the public with their meaning. They show, of course, that psychology has virtually repudiated the standards of proof that are prescribed by every-day logic, and that it has covertly adopted a 'special' (non-Aristotelian) logic by which to govern its reasoning. In this new logic, equivocation is not a fallacy, but a valid procedure.

Now in logic, just as in geometry and algebra, a derived proposition is formally valid, or 'true,' if it satisfies all the axioms and operational rules of the system. It is formally invalid, or 'false,' if it contradicts any of those standards. Its truth or falsity depends on the standards, in the same way that Sin depends on the Law. 'Without the law there is no sin, for sin is a violation of the law.' By one set of standards, the proposition may be true, by another set false; but without any standards, it would be neither true or false.

Consider now some inference derived by equivocation. By the standards of proof that govern universal science and daily practice, the inference is false; by the standards that prevail in modern psychology, it may be formally valid, or true. Conversely, a proposition may be true by the standards

of science and of ordinary discourse, but false by the standards of modern psychology. But if two kinds of discourse are governed by two conflicting sets of rules, they cannot mix; you cannot transfer propositions freely from either field to the other. Each individual proposition must be tested by the standards of the field into which it is to be imported, and then reformulated, if necessary, before it can pass through the custom-house. This is very inconvenient; its alternatives are also inconvenient; but the situation cannot remain forever as it is.

I shall eventually suggest some remedies; but before I do so, I must give some concrete examples of the manner in which equivocation is committed in modern psychology; for otherwise you might suspect that I am being convinced by my own post-prandial eloquence.

First: many authors commit this fallacy in seeking to generalize observational data; expecting to attain generality by disregarding the restrictive conditions that prevailed in the experiment. Our first illustration might be drawn from physics. The mechanical efficiency E of a machine is defined by these operations: one measures the input, or work performed upon the machine within a prescribed time; he also measures the so-called useful work that the machine performs during the same time; and then divides the useful output by the total input. The quotient he calls E.

For example, consider a water-wheel that is hitched to a grist-mill. Suppose that actual measurement shows its energy-input during one minute to be 10,000 foot-pounds, and its useful output during the same minute to be 4,000 foot-pounds. Division yields us a ratio of 4,000 : 10,000 or 40 percent, which by definition is the mechanical efficiency E of the system as a grist-mill—under restrictive conditions which include the time-rate of input. Suppose, however, that we disregard this fact, and set up the equation $E = O/I$; in which we substitute 40 percent for E. The equation then asserts generally that the useful output of this machine is 40 percent of the total input. But the generalized equation is false. It tells us that if we should reduce the input-rate

to 10,000 foot-pounds a week, or about one foot-pound a
minute, the week's output will be 4,000 foot-pounds, or about
4/10 foot-pound a minute. But every engineer knows that
the useful output for a week, and also for a minute, will then
be nothing, because the wheel will no longer turn. Someone
has likened the mathematical formulation of empirical fact
to the speech of an orator, who set his mouth into operation,
and then walked away and left it talking: unless we take care
to keep our formula *conditional*, it will probably lie. On the
other hand, if we should use subscripts in writing it, so as to
show that an input-rate of 10,000 foot-pounds a minute is
one of the conditions under which we assert it to hold, we shall
then say nothing about the output that we might get if we
should greatly vary those conditions. And nothing is just
what we should say about it.

Modern psychology contains many spurious generaliza-
tions of this type. Some of them are made about important
matters. The first one which I shall use for illustration is
shown in Fig. 1, which I have taken from Thorndike (**8, 127**).
It has been very heavily exploited by modern educators.
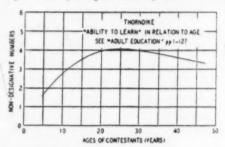
Fɪɢ. 1. Adapted from Thorndike. None of the widely separated ordinates can
legitimately be compared.

I realize that I have given this exhibit a characterization
that may sound harsh, and which should not stand unless it
stands justified. I shall therefore mention some very ele-
mentary criticisms.

First of all, the expression 'Ability to learn' should be
replaced by the expression 'Achievement in learning.' If the
experiment should be properly carried out, the expression

'Achievement' need denote nothing but the result of a defining operation, which can be, and which certainly ought to be, specified. The word 'Ability' connotes much more than experiment has ever yielded.

Secondly, the words 'learn' and 'learning' are parts of transitive verbs. So also are such words as 'memorizing,' 'forgetting,' 'retention,' and many other important psychological terms. But a transitive verb denotes a relationship between a subject and an object; and unless both related terms are specified, the name of the relationship is without meaning. The expressions 'Ability to learn' and 'Achievement in learning' imply some particular 'learners,' and some specific 'performance' by which their learning is defined.

Nothing in Thorndike's legend indicates *whose* abilities (or achievements) any particular ordinate represents, or in *what* the achievement consisted. The reader may supply these details from imagination, and unless he is both shrewd and experienced, he may assume that since the expert did not mention them in his exhibit they are not important; whereas they are essential. He is thus permitted—if not invited—to compare two or more plotted points as if they represented the achievements made at different ages by the *same learners*, in the *same kind of performance*, which is judged on the two different occasions by the same standards. The fact is that many of the ordinates to be compared represent achievements made by different persons, in different performances, which were judged by different standards; so that they are not properly to be compared at all. Of course, we might treat the curve as if it represented what we might expect of a fictitious, 'average' individual, if the comparisons represented the average achievement, made *in the same performance* by several thousand individuals in each age-group, who were selected by a constant rule; but even in those instances in which the ordinates represent the same performance, the number of individuals in each age-group is very small, and certainly not representative.

Thorndike indeed remarked (**8,** 126) that the curve really did not show what this label indicated: that in order to de-

termine the true relationship between 'learning-ability' and age, one would have to perform some defining operations that have not been performed, and which may be incapable of being performed. He said that 'no harm would result' from this equivocation, if we never forgot that it had been committed. I personally think that he was optimistic in expecting a typical reader to remember that; and Woodworth (12, 43) and Garrett (2, 12) have justified my misgivings.

These authors present well smoothed copies of Thorndike's curve. Woodworth's legend reads: ". . . The age curve of learning ability. The curve rises in the early years, indicating the growing ability *of the individual* to learn new material, and declines slowly from a maximum in the twenties." (The italics are mine.)

Garrett labels these ordinates 'Ability to learn,' and the abscissæ 'Years of age.' His legend reads: "The *general form* of the curve of ability to learn in relation to age. Note that at forty-five years of age, *people* learn as readily as *they* could when *they* were fifteen. . . ." (The italics are mine.)

Considering that the curve represents the achievements of no individuals who were tested at the age of fifteen and again at the age of forty-five, this inference is a logical feat. It could not be drawn by the rules of ordinary logic; and it goes to show how powerful a tool equivocation is. This is precisely the kind of interpretation which Thorndike warned us against —but yet employed; and it nicely illustrates the method of illicit generalization. It is only by disregard of restrictive conditions that the form of this relationship can be regarded as 'general.' Certainly, it has not been proved, in the ordinary way, that if one should assign proper arithmetic values to the constants in the equation of this curve, it will satisfactorily describe the growth, the persistence and the decay of any actual or typical individual's 'Ability to learn' whatever he may attempt. But Garrett—speaking as an expert, and as the semi-official representative of a national association of professional psychologists, and speaking to laymen who must take or reject his interpretation on faith—says that this study has "demonstrated clearly that *no one*—at least up to

forty-five—need fear that he cannot learn *anything that he wants to learn*, as well as *he* could when in his teens." (These italics, also, are mine.)

By such use of equivocation, an inference has been drawn. It passes for valid in modern psychology, although the rules of ordinary logic require us to brand it as counterfeit. Its implications are important—as they have been represented to be. A genuine experiment may eventually show whether it agrees with observational fact or not. That experiment has yet to be performed, and I do not prejudge its result. But when tendered as an inference from the observational facts which Thorndike presents, it ought to be rejected, unless we repudiate Aristotle and create an isolated field of discourse.

Another way in which equivocation is often committed in modern psychology lies in spurious identification of facts of assumption with facts of observation. For illustration, I present in Fig. 2, a so-called 'learning-curve' yielded by one
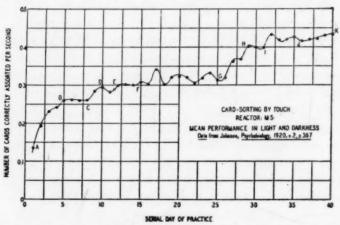
FIG. 2. To illustrate the conventional interpretation of so-called 'learning curves.'

of my subjects (4) a 20-year old woman, whom I did not consider to be exceptionally bright. She was given a well-shuffled pack of 100 playing cards, divided into four 'suits' of 25 cards each, which were denoted by simple patterns perforated in the cards, to be distinguished by touch, without the aid of vision. During her work she wore a pair of closely

fitted goggles, the lenses of which were replaced by panes of diffusing glass. At each daily sitting, she sorted the cards four times—twice in the light and twice in darkness.

The ordinate to each plotted point (indicated by a circle) represents the average number of cards which she correctly sorted in a second, while the abscissa represents the serial day of practice. The curve was drawn smoothly only for the sake of its appearance; it has no meaning except at the empirical points. The relationship is interesting of itself; but I should be very unfashionable and out of date if I should stop when I had exhibited it.

To be fashionable, I should have to say that practice reduces the resistance which the synapses in 'certain' conduction-paths offer to the passage of nervous current, below the resistance of 'certain' others; that it is this modification which accounts for the observed increase in speed in card-sorting; and that the modification which has occurred up to the end of any stage of practice is proportional to the difference between the height of this ordinate and the height of the first ordinate. I should have to say this, even though nobody has ever measured synaptic resistance, before, during and after learning; and even though nobody is now perfectly certain that in the intact living organism two consecutive nerve-cells are physically separated in their synapse. I may add that I have talked in just this fashion, and believed what I was saying—just as each of you has probably done. By 1933, we may think it queer that we ever burdened such meager facts of observation with so much unnecessary assumption—but we may let that pass. Note that in making the assumption we obligate ourselves to say that the steeper the slope of the curve is at any point, the more rapidly the resistance of these 'certain' pathways was being diminished.

For example, between *A* and *B*, between *C* and *D*, and between *G* and *H*, the slope, in the average, is comparatively steep. We must therefore say that during these times, the resistance of the synapses of the 'certain' pathways was diminishing rapidly. Between *J* and *K*, the slope in the average is gentle; hence we must say that the resistance was

diminishing slowly. Between E and F the average slope is zero; hence we must say that the synaptic resistance was neither increasing or diminishing consistently. Between B and C and between H and I, the slope is negative; during those times, so we must say, the synaptic resistance in these 'certain' pathways was increasing.

Of course, when we talk in this manner, we pretend to give information that we do not possess; we really have made no measurements that would justify us in saying anything about synaptic resistance. We could hardly justify our mention of it by calling it a 'working' hypothesis, since nobody is yet able to make the experiments that would be required to test it; nor have we any plausible reason for expecting our limitations to be overcome soon. If we could trust ourselves not to forget that all this interpretation is assumptional, we might plead that it offers a more vivid descriptive form than certain other forms—as Thorndike did with his curve of the growth and decay of so-called abilities. But I must offer an objection besides the one which I urged against him: namely, that this interpretation implies assertions that are contrary to fact.

The interpretation implies that we are dealing (by hypothesis) with the same collection of synapses; and by the most fashionable hypothesis, this could be justified only if we assumed that the physical pattern of muscular response was constant. The fact is, that it usually remains untested; but that in this case it was observed *not* to remain constant. In the interval between B and C, the reactor abandoned her former method of holding and feeling the cards, and adopted a radically different one. Were the nervous pathways that she had previously used being used now or not? Anyone who gives a categorical answer to that question should play fair, and tell us how he found it out; but in the mean time, I leave the suggestion that the new pattern of motor response may be associated with a new pattern of conduction-paths; so that we cannot say anything about the changes that may have occurred in the resistances of the older ones, when subjected to the former pattern of stimulation.

In modern psychology, a similar interpretation is placed on changes in frequency of conventional response, of distance traversed, and of calories evolved: all these so-called criteria of learning are treated as if they were mutually interchangeable in defining what is called 'synaptic resistance.'

Again: equivocation is often committed by modern psychologists, in falsely identifying formal concepts with concepts that are defined by operations. I shall mention more than one example, the first being a comparison of the treatment which Ebbinghaus gave to his own results on the 'retention' of nonsense syllables, with the treatment imposed upon these results by certain modern psychologists.

Ebbinghaus (1) read lists of 13 nonsense-syllables repeatedly, one at a time, until he could recite the list twice without error. After a prescribed time had elapsed, he rememorized the list, and considered what has been called his 'retention.' This concept is perfectly defined, by the operations which I shall describe. First, he counted the minutes n_1 which he spent in learning a given list on the first occasion (subtracting the time that he spent in trying to recite). Secondly, he counted the minutes n_2 that he spent in relearning the list—*i.e.*, on the second occasion. Next, he takes the quotient n_1/n_2, subtracts it from unity, and multiplies the remainder by 100. The product he calls 'retention' b, which obviously has this defining equation:

$$b \equiv 100(1 - n_2/n_1). \qquad (1)$$

The concept has no meaning except the result of performing these three arithmetic operations on these two measurements of time; nor did he impose any assumptional meaning upon it, as so many modern psychologists have done.

Ebbinghaus memorized many individual lists *twice*, allowing various times to elapse between the first and the second memorizations, but making those times constant for certain groups of lists, which he averaged. The interval between memorizations he expressed in minutes, and increased the expression by one minute, for theoretical reasons which may presently appear. The 'corrected' expression of the interval

he calls t. In a table he gives the values of his averages of 'retention' b, paired with the corresponding values of the 'corrected' time-interval t. This table has been reduced to graphic expression, and published in so many elementary texts that I need not reproduce the graph here. Instead, I show you, in Fig. 3, the result of plotting 'retention' b, not against t, but against its common logarithm $\text{Log}_{10}\, t$, indicating the observed values by small black circles.
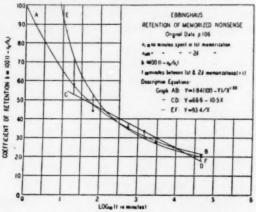
FIG. 3. Ebbinghaus's observational data on oblivescence of nonsense-syllables. Each plotted point represents the averages of varying numbers of lists of 13 syllables each, memorized and re-memorized by one reactor to two consecutive perfect recitations. The graphs represent three 'general equations' that have been proposed to describe the results.

The curve AB is the graph of the equation,

$$b = \frac{100 \cdot k}{k + (\text{Log}_{10}\, t)^c},\qquad(2)$$

which is the general form that Ebbinghaus chose. He used the values $k = 1.85$, $c = 1.25$, which he obtained by some approximative method, and which agree well enough with the values $k = 1.84$, $c = 1.23$, yielded by the method of least squares.[3]

[3] Equation (2) may be written

$$\frac{b}{100 - b} = \frac{k}{(\text{Log}_{10}\, t)^c},$$

which yields

$$\text{Log}_{10}\, b - \text{Log}_{10}\, (100 - b) = \text{Log}_{10}\, k - c \cdot \text{Log}_{10}\, (\text{Log}_{10}\, t).$$

The last equation, being in linear form, is easily solved for k and c by the method of least squares.

The straight line CD obviously represents

$$b = A - B \cdot \text{Log}_{10}\, t, \tag{3}$$

which is a general form that may be well considered for use in making a first approximation. By least squares, the same data yield $A = 66.6$, $B = 10.5$. This equation describes the experimental results somewhat less precisely than equation (2), though perhaps as closely as such results ought to be formulated, if the equation is to be used for tentative prediction.

The curve EF is the graph of the general equation presented by Garrett (3, 64, f.n.), which reads

$$b = \frac{K}{(\text{Log}\, t)C},$$

in which both K and C are constants. Obviously, their ratio $\kappa = K/C$ is also a constant. Hence, we may write the last equation:

$$b = \kappa \div \text{Log}_{10}\, t. \tag{4}$$

The method of least squares, applied to Ebbinghaus's data, yields $\kappa = 93.4$ as the 'best' value to use.

The vertical distance of any plotted point from a given curve is proportional to the error that would be committed if the observed value of b were replaced by the value predicted from the equation of that curve. Mere inspection shows that formula (2) gives a satisfactory description; formula (3) an inferior, but perhaps tolerable description; and formula (4) a bad description, since the deviations are systematic, as well as large.[4] If the formula (2) of Ebbinghaus were extrapolated to the point at which $\text{Log}_{10}\, t = 0$, at which $t = 1$, *immediately* after learning had been perfected (since t is

[4] Because of the peculiar form in which Garrett's equation was written, as well as because of its descriptional failure, one might suspect that some printer or copyist had committed an error. It looks, in fact, like a very badly garbled copy of Ebbinghaus's formula (2). But since Dr. Garrett made no such suggestion when his attention was called to the inadequacy of the formula, and since his verbal description, immediately preceding its statement, agrees with the latter and not with the formula of Ebbinghaus, I shall apply the formula as if he meant it to stand as written, in considering the results of Radosavljevich.

measured from one minute before) the result is in accordance with expectation based on experience; whereas Garrett's formula (4) yields an infinite value of b at that point, and formula (3) a value less than 100. Nevertheless, within the range of experience, formula (3) does fairly well; and extrapolation is certainly not called for, at the least.

So much for graphic representation and mathematical formulation of these important results of Ebbinghaus. Now for some modernized interpretations that have been put upon them.

Several modern authors have pointed with pride to the really meritorious study of Ebbinghaus, saying that it has established what they call a 'general law' of retention and forgetting, and thereby proving that psychology is worthy of classification among the exact and quantitative sciences. To appraise their assertion, we should first ask what they mean by the little word 'general.' It has, in fact, two principal meanings, which we dare not interchange in the same discourse. In the language of mathematics and of exact science, it means 'subject to no exceptions.' In the language of social conversation, and of vague, listless, and slovenly description, it means 'subject to exceptions.' For example, consider the assertion, 'Red-haired women, in general, are high-tempered.' In the language of exact description, the assertion means that red-haired women, without exception, are high-tempered. In the language of slovenly description, it means that all red-haired women are high-tempered except those who are not. It is impossible to make an assertion more vague than that. Its extent is indeterminate; one cannot tell whether it includes all members of the subject-class, some members, or none. One can truthfully affirm any property whatsoever, of any subject, if one makes his assertion 'general' in the slovenly sense of the word. In that sense, one may truthfully say that red-haired women, 'in general,' are black-haired, since all of them are black-haired except those who are not.

Now some portions of modern psychology are written in the language of exact description; while other portions are written in the language of slovenly description; so that we

have to notice what language is being employed in each particular place. One might expect that when the writer presents the results of highly refined methods of measurement, which have undergone elaborate statistical treatment; and when he uses mathematical symbols, mathematical operations, and mathematical formulations, he would also use verbal expressions borrowed from mathematics in their exact sense; but a survey of the writings of the most typical modern psychologists would quickly remove that expectation.

Ordinarily, if a writer says that a certain set of results fall into a certain 'general' relationship, or follow some 'general' law, one would assume that he meant, that the law will describe *any* similar set of results with satisfactory precision, if one assigns the proper arithmetic values to the constants in the 'general' descriptive equation. Three equations have been proposed for description of the results of Ebbinghaus: namely equations (2), (3) and (4), as given above. Equation (3) describes the findings with about as much precision as the psychologist usually demands; equation (2) with more precision than that, and equation (4) with less. Does any of these descriptive equations state a general law of retention; and if so, in what sense is the law 'general?'

Certainly, any equation is 'general' in the slovenly sense of that term, for it will describe all experimental results satisfactorily, except those which it will not. But is any of these equations 'general' in the scientific meaning of the term?

Let us not demand too much of such a general law. We should not expect it to describe a white rat's 'retention' of the shortest true path through a maze, or an adult's 'retention' of the content of the lectures of his professor. These 'retentions' are defined by other operations than those which define the concept of Ebbinghaus; they are therefore different concepts from his; and we have no reason, prior to experiment, for expecting his descriptive law to apply to them. To be sure, certain modern psychologists have deliberately aroused such an expectation in elementary students of psychology and education, and in the untutored listeners to

popular talks on psychology, but we need not follow them. Let us ask, however, whether any of these descriptive formulas is general, to the extent that it will satisfactorily describe the results of a similar experiment, made upon other subjects, who had to memorize similar material, under conditions which differed somewhat from those of Ebbinghaus.

In Fig. 4, I show the result of applying these three general
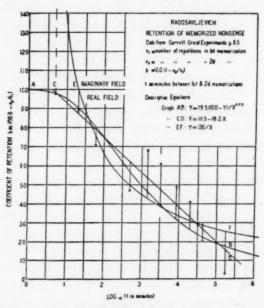
FIG. 4. Radosavljevich's observational data on oblivescence of nonsense-syllables, with graphs of three 'general equations' proposed for description. The descriptive equations should read as follows:

Graph AB: $Y = 19.5(100 - Y)/X^{2.73}$ (Ebbinghaus)
" CD: $Y = 111.5 - 18.2\,X$ (Johnson)
" EF: $Y = 136/X$ (Garrett)

formulas to the mean results of the subjects of Radosavljevich (7). I have relied on Garrett's tabular presentation (3, 65) of these data, since I could not procure a copy of the original monograph at the time I needed to use it. The constants for all three equations were derived by least squares from these data, and are shown in the legend to the figure. It can be readily seen that Garrett's formula (4) fails, *within the range*

of experience. It predicts that the value of b will be greater than 100 if t is less than some 22 minutes, so that its logarithm is less than 1.34. But this prediction is not only at variance with fact, as the plot of the observed results reveals; it also contradicts the defining equation (1), which reads

$$b \equiv 100(1 - n_2/n_1).$$

This equation asserts that b cannot exceed 100; for if it did, the ratio n_2/n_1 would have to be negative, which implies that one of its terms n_2 or n_1 is negative. But both n_2 and n_1 denote the number of times the experimenter exposed the list before the subject gave the required number of perfect recitations on the two occasions;[5] consequently neither term can be negative. The simplest of the three formulas, namely (3), gives the closest description of all; but mere inspection shows that none of the three is satisfactory.

Thus we may see that Ebbinghaus was justified in his scientific modesty and reserve. Although he formulated his actual findings quite accurately, he regarded the formula as being nothing more than a brief notation of his findings made at that time (1, 107). He remarked rather earlier (1, 105), that nobody would wish to find such a general law in data that had been yielded by a single reactor, in memorizing one kind of material, by one specific method, and under highly specialized conditions. His modern interpreters might do well to adopt his cautiousness.[6]

[5] Radosavljevich, according to his modern reviewers, exposed each syllable for a constant time, and desisted as soon as the subject recited the list acceptably; hence the values of n_1 and n_2 are proportional to the number of minutes that were expended in memorizing and rememorizing, respectively. Their ratio has the same meaning as in the formula which defines Ebbinghaus's concept of 'retention' b.

[6] Bei dem speciellen, individuellen und noch dazu unsicheren Charakter unserer Zahlen wird man nicht gleich das „ Gesetz " zu wissen verlangen, welches in ihnen zur Erscheinung kommt. Immerhin ist es merkwürdig, dass sich alle 7 Werte, welche eine Zeit von 1/3 Stunde bis zu 31 Tagen (also vom einfachen bis zum 2000 fachen) umfassen, mit leidlicher Annäherung einer ziemlich einfachen mathematischen Formel einfügen lassen.—(S. 105)

In Worten: wurden dreizehn silbige sinnlose Reihen auswendig gelernt und nachher nach verschiedenen zeitlichen Intervallen wieder gelernt, so waren die Quotienten aus den hierbei ersparten und den hierbei gebrauchten Arbeitszeiten annähernd umgekehrt proportional einer kleinen Potenz der Logarithmen jener zeitlichen Inter-

21

In Fig. 5, I show the result of applying these three 'general' formulas to the mean results of the subjects of Radosavljevich in the memorization of poetry. These data, also, are taken from Garrett's table (3, 65). The constants, computed by least squares, are shown in the legend of the
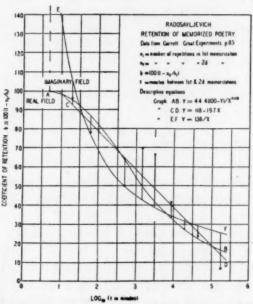

FIG. 5. Radosavljevich's observational data on oblivescence of memorized poetry, with graphs of three 'general equations' proposed for description. The descriptive equations should read as follows:

$$\text{Graph } AB: Y = 44.4(100 - Y)/X^{2.38} \quad \text{(Ebbinghaus)}$$
$$\text{''} \quad CD: Y = 118 - 19.7X \quad \text{(Johnson)}$$
$$\text{''} \quad EF: Y = 136/X \quad \text{(Garrett)}$$

figure. All were derived from the same data, except that in computing the constants for Ebbinghaus's general formula (2), it was not possible to use the observed value $t = 5$

valle. Oder kürzer und ungenauer: die Quotienten aus Behaltenem und Vergessenem verhielten sich umgekehrt wie die Logarithmen der Zeiten.

Natürlich hat dieser Satz und die ihm zu Grunde liegende Formel hier keinen anderen Wert, als den einer kurzen Notierung der obigen, unter den beschriebenen Umständen gefundenen, einmaligen Resultate. Ob sie darüber hinaus eine allgemeinere Bedeutung besitzen, wo dann die Verschiedenheiten anderer Umstände oder anderer Individualitäten in anderen Konstanten ihren Ausdruck finden würden, kann ich einstweilen nicht ausmachen.—(S. 107)

minutes, $\text{Log}_{10}\, t = 0.7$, $b = 100$, because the logarithm of that value of $b/(100 - b)$ is infinite. However, if we compute this value from the descriptive equation derived from the remaining data, the result differs from observation by a value that is too small to show on the graph. As in the preceding exhibit (Fig. 4), we see that Garrett's formula is not applicable, since it predicts impossible values of b within the range of experience: namely values that exceed 100 for all values of t which are less than 23.5 minutes, their common logarithms being less than 1.37.[7] Again, we find that the simplest formula (3) gives the closest description; but that none of the three is tolerable.

I know of no other general descriptive equation that has been applied to the data of these three experiments. I am therefore waiting to be convinced that the experiment of Ebbinghaus has yielded a truly 'general law' of learning and forgetting; and I wish the emancipated psychologists would stop talking about general laws of this kind until they exhibit those laws and justify them, or else teach us not to believe what they appear to be saying.[8]

I do not doubt that a general equation could be formed, which would contain at least four constants, and which would

[7] In the language of the popularizers of modern psychology, we may say that according to Garrett's formula, during the first twenty-odd minutes after these reactors had memorized a batch of material, they remembered more of it than they had learned.

[8] It is interesting to note that some modern psychologists are seeking for *simple* 'laws' of forgetting, in organisms that do not observe simple laws in learning. Some commentators on the results of Radosavljevich seem to be worried because his observed values of b were so much greater after 24 hours than after 8 hours—as if 'retention' ought to decline at a regular rate unless it was interfered with. Well, the bodily mechanism that is involved appears to be a rather complicated system, which has a variety of equilibrium-states, and which may shift suddenly from one of these states to another when it is subjected to some particular pattern and sequence of disturbances. Moreover, we know that it is continually being played upon. A physicist who considered a comparatively simple system that is characterized by hysteresis would certainly not expect it to return gradually to a former state of equilibrium if it were being excited during its period of recovery. He would expect it rather to behave according to one law for a time, and then suddenly adopt some other law. I suggest this as an alternative, in connection with the results of Radosavljevich shown in Fig. 4. The points can be plotted very closely to three straight lines, which are nearly parallel to each other. The plot reminds one of observations that have been made on other kinds of behavior, in non-living hysteretic systems.

describe the findings of all three experiments closely. If it contained enough constants its graph would pass through every plotted point. I will concede that the most typical psychologist might solve the equation in less time than he would need to repeat the experiment; and perhaps in less time than he would require for checking the averages and statistical constants of the original author. But I will not concede that such a formula could be applied 'generally' to other subjects, or to other kinds of learning, or to other conditions of learning, or to other criteria of forgetting. I question whether it could be relied upon in the prediction of what the same memorizers, of similar material, and under similar conditions, would show outside the range of experience. In other words, I would not trust its extrapolation or interpolation.

Modern psychology exemplifies another interesting way of committing equivocation, in another fashionable interpretation of the experiment of Ebbinghaus which it contains. I repeat the defining equation of the concept which Ebbinghaus called 'retention' b:

$$b \equiv 100(1 - n_2/n_1).$$

I repeat that the concept is defined by three arithmetic operations performed on two measurements of time. C. Burt (**2**, 14) says that it measures the 'percent remembered' of that which was memorized. Garrett (**2**) calls it a measure of the "amount which 'stuck'" during the interval between the first and the second memorization. I must now point out a slight inconvenience that results from that manner of speaking. As long as the subject requires a smaller investment of time (or presentations) for the second memorization than for the first, the quotient n_2/n_1 is less than unity, and the 'coefficient of retention' b is positive. But suppose in some particular experiment the subject invests more time (or presentations) in the second memorization than in the first. The quotient n_2/n_1 is now greater than unity, so that the 'coefficient of retention' b is negative. As long as we adhere to operational definition of b, as Ebbinghaus did, the finding

creates no new difficulty: a negative value of b implies nothing except that actual counting proved n_2 to be greater than n_1, and there should be nothing disconcerting about that. But the assumptional rule of description which Burt and Garrett employ requires them to say that the subject remembered less than nothing of what he had memorized; that less than nothing of what he had gained by his efforts to learn had 'stuck' during the interval between his two efforts. I have wondered what such an assertion might mean, but I haven't yet found out. I confess that I have not seen a report of an experiment just like this one, which yielded such a negative result, but there is nothing, as far as I can see, by which a negative result might be precluded. In more casual experiments in what modern psychologists would call 'relearning,' I have obtained just such results occasionally, and I suspect that each of you could match the experience. In his averages of values of b which correspond to moderate and short time-intervals, Ebbinghaus included some individual values that were rather close to zero; I should say that an occasional negative value is to be expected, even in this type of study. The question, however, is this: shall we adhere to operational definition, or, by imposing equivocal meanings on the results of the defining operations, build up a clumsy system of interpretation that we may have to wreck and rebuild at any time that our experience is enlarged? Bridgman has shown the disasters that such equivocation has wrought in modern physics, and we should not expect psychology to escape them.

In Thurstone's (10) conception of an 'absolute zero' of test-intelligence, and in his discussion of the so-called laws of mental growth, one finds a similar dependence upon equivocation, in that he interchanges operationally defined concepts with formally defined concepts that are inconsistent with them though called by their names. In Fig. 6 I represent the 'law' by which he conceives National-Test intelligence to be growing. This law has passed for a very important discovery: namely, that 'the mental functions which are operative in the adult intelligence tests begin their development at birth.' (10, 195.)

To detect the interesting fallacy that underlies this argument, one must examine the definitions (which are operational) of two of his concepts: namely, 'Mean Test-Performance,' and 'Absolute Zero.'

The operations which define his 'Mean Test Performance' are exhibited in an earlier article (9). To illustrate them I
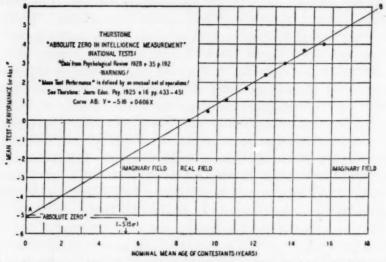
FIG. 6. Thurstone's conception of the law of growth of National Test-Intelligence.

shall refer to the so-called National Intelligence Tests (*13*), since it is their results which most favor Thurstone's argument. They require each contestant to return *a set of written answers to a set of printed questions*. Let us not forget that all which follows is based upon that. Thurstone first segregates the contestants into age-groups, according to their ages at the last birthday, and applies the following procedure to each age-group separately.

(1) Count the members, and call their number N.

(2) Denoting the individual questions by numbers, count the members of the group who answer each question conventionally, denoting the result by the symbols n_1, n_2, n_3, etc.

(3) Take the quotients n_1/N, n_2/N, n_3/N, etc.

(4) Reject those questions for which the value of $n/N = 1$

or o; *i.e.*, those which *all members* and those which *no members* of the age-group answer conventionally.

(5) From a table of the probability-integral expressed as a function of the argument x/σ_z, take the values of the argument which correspond to the several values of n/N, designating them by the symbols X_1, X_2, X_3, *etc.*, in which the subscripts still denote the individual test-questions.

(6) Find the arithmetic mean $m \equiv (1/N) \cdot \Sigma X$ of the several values of X.

(7) Find the standard deviation $S \equiv \sqrt{(1/N) \cdot \Sigma(X - m)^2}$ of the several values of X from their own mean.

Having obtained these findings for all the age-groups that are to be considered, perform the following operations for each age-group separately:

(8) Find the value of the constant which Thurstone arbitrarily calls σ. If we choose the symbol a to denote the nominal age of some particular group, and the symbol $a + b$ to denote the nominal age of *any* group whatever (b expressing the deviation of its nominal age from a), then, from Thurstone's definitions,

$$\sigma_{a+b} \equiv S_a/S_{a+b}.$$

Note, that for any age-group, what Thurstone calls its 'absolute variability,' or its 'absolute standard deviation' σ, as it was just defined, is *inversely proportional* to the standard deviation S of its X-values from their own mean.

(9) Find the 'Mean Test Performance' for each group. If its nominal age is $a + b$, the defining equation of its Mean Test Performance is

$$M_{a+b} \equiv m_a - \sigma_{a+b} \cdot m_{a+b},$$

which may be written

$$M_{a+b} \equiv m_a - (S_a/S_{a+b}) \cdot m_{a+b}.$$

The sole meaning of the expression 'Mean Test Performance' is the result of these defining operations.

Let us now examine Thurstone's concept of the 'absolute zero' of test-intelligence. Its definition is simple. One pairs

the value of M for each age-group with the corresponding value of σ, and assumes the numerical equivalence

$$M = f(\sigma).$$

One now chooses some general form of expansion of the function $f(\sigma)$, such that it will describe the observed data with satisfactory precision, provided one assigns proper values to its constants. Having determined the proper values, and effected the expansion, one equates $f(\sigma)$ to zero, and solves the equation for M. *By definition*, this value of M is the absolute zero of test-intelligence for the test that is considered.

For example, in dealing with the National Tests, Thurstone (**10**) assumed $M = f(\sigma) \equiv A + B \cdot \sigma$, in which he set $A = -5.15$, $B = 4.985$. Thus he places the absolute zero of National-Test intelligence for any age-group at 5.15 times its own σ-unit below its own value of M.

Against this procedure, two important objections lie. First, it depends upon the extrapolation of a descriptive formula, which is but approximate throughout the range of experience, through a range that exceeds the range of experience. But numerous equations can be chosen, each of which will describe the observed facts *perfectly*. Extrapolation of some of them will define 'absolute zero' as being much greater than -5.15σ; extrapolation of others will render it much smaller than this; extrapolation of some will locate it at $-\infty$, and an interesting speculative meaning could be assigned to such a fact. The choice of a general form of descriptive equation is arbitrary; so must be the result of its extrapolation.

Secondly, in such intelligence-tests as the National, the 'Mean Test-Performance' M is defined, as we have just seen, by a chain of operations that are performed upon a classification and census of written answers to printed questions (operations 1–9 above). The concept M is identical with the results of those operations. It is illicit to interchange it with any formally defined concept that lacks any properties which are implied in those results, or that has properties which

are not implied in them. Thurstone makes just such an interchange, and thereby performs a fallacious inference that looks important. He uses the symbol 'Mean Test Performance' M to denote indifferently two different concepts. The first concept is defined by operation 9; the second concept is defined by the solution of an equation which asserts that the result of operation 9 is approximately equivalent, numerically, to some function of the nominal age of the contestant-groups. The graph of one such equation is the line AB in my Fig. 6. It asserts that the values of M for age-groups whose members are too young to read and write are nevertheless determinate; whereas the defining equation (operation 9) asserts that they are indeterminate. (For, since no member of such a group could return any conventional written answer to any of the questions, each question would yield $n/N = 0$, $X = -\infty$, so that operation 9, and indeed all the operations after the fourth, would be either meaningless or impossible.)

In most discussion of the measurement of personal traits, one finds the same sort of equivocation. What is ordinarily called 'intelligence' by mental testers is defined by a perfectly specific and evident set of operations. One counts the conventional answers that each contestant returns to a set of questions, and reduces the count to some relative basis for comparison. But the highly modernized psychologists then talk about the results as if they were measurements of something that they call 'general ability,' 'innate learning capacity,' something that has never been measured, or even defined; but of which they say each individual receives a fixed quantity at birth, that quantity being (like the number of the Elect), 'so certain and definite that it cannot be increased or diminished.' I assert that this concept is not the one that was defined by these operations; that nothing but extreme confusion has resulted from equivocal interchange of the two; and that moreover, no practical use to which the results of these tests can be put is in any way related to this illicitly imposed meaning.

The graphologist makes note of the long tails which some writers put upon the final letters of words, and of the long

cross-bars which they put upon their *t*'s. These are signs, so he tells us, of an unusual degree of 'persistence.' So they are—of persistence in these motions with the hand and pen. If you doubt the fact, all you need to do is to apply a milli-meter-scale to the lines, strike an average, and compare it with averages of similar measurements made on the script of other writers. This writer is so persistent that it is hard for him to stop moving the pen once he has started it in these particular directions. We, as professional, and scientific psychologists, laugh when the graphologist tells us that these persistencies are signs of 'general' Persistence—persistence in other activities also. And we are right. His persistences in other activities are defined by other measurements. Such persistences have never been measured and correlated with persistence in this task; hence one's graphological persistence cannot be used as a test of persistence in other activities. But is his reasoning any less dependent on equivocation, or by Aristotelian standards, any more fallacious, than our reasoning when we attempt an illicit generalization from the results of the tests that we give? I think that a candid survey and analysis of our own doctrine will show that it is not.

For example, J. S. Szymansky required his subjects to spend from 1/5 to half an hour sitting in a chair that was mounted so that the movements of the occupant would displace the recording pen of a kymograph. He instructed his subjects to refrain, as long as they could, from changing their bodily positions during this time. Nevertheless, they did move—rather often. The average interval between suc-cessive movements, for a group of 16 subjects, was of the order of 1.8 minutes. Nevertheless, individual subjects differed widely among themselves in the average time during which they refrained from stirring. Now to inhibit an impulse to stir, when a stir will relieve a large pattern of organic irritation, requires effort. Obviously, the average time during which an individual refrained from moving is a direct measure of his 'perseverance'—*i.e.*, in the task of sitting still. Experi-ment shows that, clearly. Consequently—so reasoned Szy-mansky—they are indirect measures of *General Perseverance*,

and will enable us to estimate with tolerable accuracy how long the subject will persevere in any other activity that you may select, and in the face of any annoyances that necessarily accompany that activity. This is inference by means of equivocation, of course; he thus employed the logic of the graphologist, which is identical with the logic that is employed by the mental testers, whose ranks Szymansky intended to join.

But I may have said enough about the logic of mental testing in particular. Some of my friends who are working in this field think that on other occasions (**5**, **6**) I may have said a little too much. They accept my criticisms of the logical structure of the program; but they fear that I have disparaged the practical value of their achievements by not excepting it from condemnation of the logic. Very well. They may have accomplished something of value, even if they have worked no miracles. I have no wish to disparage that; it can be judged on its merits; and I shall gladly keep the peace if only they will clean up their logic. That logic is not unique; my present complaint is that it has pervaded the whole of modern psychology, and all the other biological and social sciences; nevertheless it is a stench, a reproach and a byword in any field in which it may be employed, as long as the workers in that field proclaim their allegiance to the logic of science and common sense. If I have been more diligent than others in giving a name to their vices, and in exhorting them to repentance, I am not the only one who appreciates their peril.

This brings me to a final illustration of equivocation. It lies at the foundation of orthodox behaviorism. My distinguished teacher, Dr. Watson, correctly points out that introspectively observable sensations cannot be exhibited to any person except their owner. Most of the subjective psychologists likewise assert in their introductions that sensations are not exhibitable; while in their chapters on conditioned reflexes and association they virtually deny that non-exhibitables can be represented. They thereby deny that sensations can be inferred; whether from changes that

occur in the environment, or from the responses of the subject. This implies that introspectional sensations cannot be treated as data of common observation; and the conclusion justifies the methods of the objective psychologist. Introspectional sensations may or may not exist, but if they do exist, they cannot become the subject-matter of common observation, either direct or indirect. This reasoning is valid; but when one declares that anything that cannot be exhibited, or inferred from data of exhibition, does not *exist*, he commits equivocation, by treating one of two distinct tests of existence as if it were identical with the other. The result is a beautiful example of rhetorical luxury, which reminds one more of the pulpit than of the laboratory, and in which some objective psychologists have not cared to join.

Equivocation, or illicit identification, can be committed even though the two concepts which are confused have more in common than a name. In Aristotelian logic, two concepts cannot be treated as identical unless they have in common *all* the properties that are considered in the discussion. In the special logics, by one of which modern psychology appears to be governed, two concepts are identified if they have *any* property in common. When we first commit equivocation, we may demand only that both concepts should have a common name. As we become more rigorous, we demand that they have other properties in common. For example, the savage, who resorts to Magic, wishes to bring ruin upon his enemy. He cannot get at the enemy or perhaps he dare not; so he makes a doll, which he invests with the name of his enemy. He may have to hire a very powerful sorcerer, and use a very elaborate ritual, to fix the name of his enemy upon the doll. Now, he may curse the doll, beat and cuff it, burn it in the fire—and in so doing, injure his enemy. Thus far he has committed an equivocation, in so patent a form that even he can recognize it. He therefore seeks to invest the doll with other properties of his enemy. First, he makes it resemble the enemy as much as he can with his limited skill in sculpture. Next, he dresses it like the enemy, borrowing or stealing some fragments of the enemy's clothing for the

purpose; or, if he cannot do this, he imitates these garments as far as he may. But this does not satisfy him. He is striving for perfect identification, so as to make his curse more nearly effective. So, if only he can procure some hair, or a bit of nail, from the enemy's body, the curse will work better. His image now has in common with the enemy the properties of name, personal appearance, garb, and fetich, namely hair or nails; and since our savage is imaginary, we may as well imagine that he is striving to make the image share the maximum number of properties with the enemy, so that he will really abuse the enemy in abusing it. In other words, our notions of identification may be evolutionary; nevertheless, Aristotle and his true disciples insist that nothing short of complete community of properties can render two concepts identical.

Well, the psychoanalyst has approached the scientific and practical standards of identification more nearly than some of the other emancipated psychologists have done; for at least, he sometimes demands that two concepts which he identifies should have more in common than a name. Nevertheless, he also uses a special logic, which isolates him from the world of science and of common sense. We all admit that parental love and sexual passion have some properties in common. Each of them has also some properties which the other does not share. By disregarding the differences, and considering only the resemblances between these two concepts, the psychoanalyst renders them 'identical.' So they are—in the logic of fairyland—not in any logic that can be put to practical use. Through illicit identification, the psychoanalyst has concluded that the whole human race yearns for adultery, for incest, for pederasty, for divinity, for the loss of personal identity in union with something or other, and for annihilation and death; and that the chief trouble with the race is that it does not recognize these yearnings and call them by their true names. These doctrines are presented in the conventional text-books of modern psychology. Usually, they are not fully endorsed, but are considered respectfully, with the remark that they contain a

great deal of truth, even though they may be over-stated. I dare say that no author who adhered to Aristotelian standards would present them except as logical curiosities.

Old-fashioned psychology was certainly not dangerous; it was not important; it was hardly interesting. Modern psychology is. It is interesting, because it deals with the most important human relationships. It is important, because it endeavors to explain those relationships, and by explaining, to modify them. It is dangerous, because its reasoning upon the facts is so heavily grounded in fallacy. I can think of only two possible courses; between them, we probably shall be compelled to choose.

If we intend deliberately to reason by special rules, and thereby attain conclusions which will not stand when tested by the logic of science and common-sense, let us say so. Let us then exhibit those rules, and adhere to them, just as a non-Euclidean geometer exhibits and adheres to axioms which contradict the axioms of Euclid. That will yield us a logic which should be internally consistent, if the axioms and rules do not contradict each other—although I suspect the result would be terrifying. Thereafter, some expert logicians could reformulate these doctrines, taking them up one inference at a time, and render the restatements acceptable in science and in practical affairs. This would be very difficult, but yet possible. Meanwhile it cannot be done at all.

The other course is to reaffirm the standards of ordinary logic, submit every inference in modern psychology to test by them, and put out the result as something that will practically hold. The result, I dare say, will be treatises that are very much smaller than the ones we now have; our lists of solved problems will be greatly shortened; our lists of spurious problems, of absurd requirements, and of unsolved genuine problems will be lengthened; but the result will be both pleasing and valuable.

In the meantime, let us admit the facts. Our field is now so thoroughly muddled that we should warn the outsider that he dares not adopt any of our doctrines by an act of faith. If he were to examine the derivation of most of them, and

demand that they satisfy the ordinary standards of proof, he could justly tell us, 'That may be true in psychology, but it won't go here.'

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MULTIPLE FACTORS VS. TWO FACTORS AS DETERMINERS OF ABILITIES

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I. INTRODUCTION

One of the most interesting recent developments in the psychology of individual differences is the attempt to estimate the factors causing variation in abilities. The method is first to examine the intercorrelations between abilities and by mathematical means to determine the number of factors sufficient to lead to just the particular set of intercorrelations observed. The second step is to affix to each factor some psychological label, such as 'mental energy,' 'verbality,' 'memory,' 'cleverness,' 'scientific interest,' etc. Regarding the first step in this type of analysis, namely, the calculation of the number of factors (as well as of the weights of each factor in each ability), the fact that ordinarily an infinite number of factor patterns may be postulated, each consistent with the intercorrelations found, is a bit disquieting. One must, therefore, arbitrarily choose the pattern which pleases him best. Some psychologists (*e.g.*, C. Spearman) choose one general factor plus enough group and specific factors to lead to consistency. Others (*e.g.*, T. L. Kelley, L. L. Thurstone) choose the most parsimonious set, though there is here a difficulty in deciding which set is most parsimonious. Still others (*e.g.*, G. H. Thomson) choose a very large number. Each person chooses his number, not on mathematical grounds, but on the basis of his own psychological bias. Since each factor may be thought of as a 'faculty,' those who choose one general factor and those who choose a small number do so because they think that one great monarchic faculty or, on the other hand, at least a few such great faculties, are the causal agents in determining abilities. The bias of the present writer, akin to that of Thomson's, is some-

324

what the reverse, the great mass of findings in genetics and in learning inclining him towards the view that multiple independent genetic and learning factors are really the determiners of abilities.

Having thus taken his stand for innumerable factors or faculties (without as yet attacking the second problem of giving psychological names to these factors), the writer must investigate the claims of those who assert the existence of a few large ones. The most well-known come from the so-called two-factor school, which in terms of its originator, Spearman, claims to be 'the very Cinderella' of schools, 'A School to End Schools,' (**25**, p. 339), the views of which 'constitute a Copernican revolution' in psychology (**19**, p. 325), and which 'appear to be no longer seriously disputed by any psychologist of authority' (**25**, p. 339). To the task of investigating the claims of this school this paper primarily addresses itself.

II. The Two Factor Theory
A. Statement of the Theory

This theory is best presented in the words of its originator:

"Briefly stated the main, or general, theory is that every measurement of each ability can be resolved into two factors, of which the one is general, whereas the other is specific. Otherwise stated, all abilities involve one and the same factor 'g,' whereas every different ability has its own particular factor 's.' Of course, the case may arise where abilities are only different in part; they are then said to 'overlap,' or contain a 'group factor.' But, in general, this only occurs when the abilities are 'very closely and obviously allied.' We may add that the g appears to be functionally indivisible, whereas the s can usually be split up." (**21**, p. 561)

Stated in full equational form, this means that, for any four abilities, say,

$$
\begin{aligned}
x_1 &= \alpha_1 a + \beta_1 b \\
x_2 &= \alpha_2 a \qquad\quad + \gamma_2 c \\
x_3 &= \alpha_3 a \qquad\qquad\quad + \delta_3 d \\
x_4 &= \alpha_4 a \qquad\qquad\qquad\quad + \epsilon_4 e,
\end{aligned}
\tag{1}
$$

22

where a stands for g, the general factor; b, c, d, e, stand for specifics; the Greek symbols stand for weights. All variable measures are in terms of *standard scores*, and the intercorrelations between the specifics and general factor are zero, *i.e.* the factors are independent.

In briefer notation, equations (1) may be written:

$$x_1 = f(a, b) \qquad x_3 = f(a, d)$$
$$x_2 = f(a, c) \qquad x_4 = f(a, e). \qquad (2)$$

The presence of a group factor, m, between, say, x_1 and x_3, would be expressed as:

$$x_1 = f(a, m, b) \qquad x_3 = f(a, m, d). \qquad (3)$$

B. Correlational Results Expected on the Two Factor Theory

As shown by Garnett (5, p. 96), the factor pattern in (1) or (2) gives the intercorrelations:

$$r_{12} = \alpha_1\alpha_2 \qquad r_{14} = \alpha_1\alpha_4 \qquad r_{24} = \alpha_2\alpha_4$$
$$r_{13} = \alpha_1\alpha_3 \qquad r_{23} = \alpha_2\alpha_3 \qquad r_{34} = \alpha_3\alpha_4. \qquad (4)$$

These intercorrelations set up as the familiar tetrad differences, using Kelley's t notation (11, p. 47) are:

$$t_{1234} = r_{12}r_{34} - r_{13}r_{24} = \alpha_1\alpha_2\alpha_3\alpha_4 - \alpha_1\alpha_3\alpha_2\alpha_4 = 0$$
$$t_{1243} = r_{12}r_{43} - r_{14}r_{23} = \alpha_1\alpha_2\alpha_4\alpha_3 - \alpha_1\alpha_4\alpha_2\alpha_3 = 0 \qquad (5)$$
$$t_{1342} = r_{13}r_{42} - r_{14}r_{32} = \alpha_1\alpha_3\alpha_4\alpha_2 - \alpha_1\alpha_4\alpha_3\alpha_2 = 0.$$

Thus if the causal scheme is as in (1), then all the tetrad differences involving the 'true' r's (*i.e.* free from sampling errors) will vanish exactly. The value, 0, and no other value than 0 follows *necessarily*.

C. So-called 'Reversibility of Proof'

But what we really wish to know is the answer to the reverse question:

If the observed intercorrelations (assume for sake of argument that they are free from sampling errors) actually found in a given investigation give tetrad differences which vanish, then may we say that the two factor pattern and no

other pattern is the one which could produce these vanishing tetrads? Do vanishing tetrads constitute a water-tight proof of two factors?

On this matter the two-factorist can be charged with a certain amount of equivocation. Thus in *Abilities of Man* (**18**, p. iii) Spearman writes, regarding the "problem, as to whether, when the criterion was satisfied [*i.e.*, all *t*'s vanish] then every variable would *necessarily* be divisible into the said two factors," that he considers "this problem . . . solved . . . affirmatively" both by Garnett and himself (italics mine). The term, *necessarily*, apparently leaves no room for doubt on the matter. Again, he says, "the most rigorous proof has been furnished by Garnett and myself that whenever . . . the criterion of tetrad differences is satisfied, . . . the single general factor *must* occur" (**19**, p. 324, italics his). Likewise in the chapter labelled "Proof [*sic!*] that *g* and *s* exist" (**18**, Chap. X), he considers that he is bringing forth "experimental work . . . of unimpeachable quality and overwhelming quantity" which in effect is "to prove definitely whether these two factors, *g* and *s*, do or do not actually exist" (p. 137).

The fact is otherwise, however, for the vanishing of the tetrads constitutes in no sense a *proof* of the existence of *g* and *s*. The mathematical manipulations of Spearman (**18**, p. iii) and of Piaggio (**16**, p. 56) show merely that when the tetrads vanish, the variables *can* be expressed in terms of two factors. On the other hand, the variables can likewise *always be factored into multiple independent factor patterns*, general or otherwise. This fact, which we shall see more clearly later, makes it obvious that vanishing tetrads do not constitute a unique *test* of the two factor theory.

But we have so far assumed that the tetrads actually *do* vanish in practice (apart from sampling errors). But if they do *not*, and we shall later show that they do not, then the abilities cannot possibly be factored into *g* and *s*, and some sort of multiple factor pattern must be chosen from the infinite number of possibilities available.

To sum up here, we must say that the fact that the

tetrads vanish, constitutes no proof of the two factor theory since many multiple factor patterns would lead to just this same result. The satisfying of the criterion has significance in the sense only that it gives the two-factorists an opportunity to claim the two factor theory as being one possibility among many others.

III. MULTIPLE FACTOR THEORIES
A. Statement of the Theories

Since we shall later examine the correlational results from various experiments, to observe if they substantiate the contention of the two-factorists that the tetrads vanish, it is desirable to present here those multiple factor theories the claims of which are that one would expect the tetrads *not* to vanish. Whichever way the results go then, we shall have theories at hand to 'explain' them.

From his work with school children, Kelley is led to postulate the existence of such faculties as verbal, arithmetic, memory, spatial, and speed abilities (11). The statistical methods he employs permit him to show as high degree of consistency of his results with his theory as he wishes. With similarly water-tight statistical techniques, Thurstone postulates multiple factors in several sets of data on which he has worked, generally finding in each case several large faculties consistent with his results (34). For different problems, however, he does not assume that his faculties are the same. Quite differently from Kelley and Thurstone, Thomson assumes abilities to be samples (in different proportions) of the total number of elementary capacities available to the individual (2, Chap. X). He has not, it appears, as yet given an extensive psychological interpretation of these elementary units. The present writer holds a view similar to that of Thomson's, believing that abilities are determined by a large number of more or less independent factors. These factors on the hereditary side are *genes*, and on the environmental side, they are the innumerable conditionings or associations formed in the course of learning. Each ability can hardly be said, however, to 'sample' these factors

randomly in a circumscribed proportion, since each situation will call into action certain ones of them and *never* others. This is, perhaps, a theory somewhat akin to that cursorily presented by Hull in his *Aptitude Testing* (**10**). To a more extended consideration of these theories we shall devote our interests in a subsequent paper.

In terms of our briefer notation of equations (2) representing equations (1), the multiple factor hypotheses may be represented, for four variables, as follows:

$$\begin{aligned}
x_1 &= f(\Sigma l_{1_1},\ \Sigma b_1,\ \Sigma c_1,\ \Sigma d_1,\ \Sigma m_1,\ \Sigma n_1,\ \Sigma o_1,\ \Sigma a_1) \\
x_2 &= f(\Sigma l_{2_1},\ \Sigma b_2,\ \Sigma e_2,\ \Sigma k_2,\ \Sigma m_2,\ \Sigma o_2,\ \Sigma t_2,\ \Sigma a_2) \\
x_3 &= f(\Sigma l_{3_1},\ \Sigma c_3,\ \Sigma e_3,\ \Sigma u_3,\ \Sigma m_3,\ \Sigma n_3,\ \Sigma t_3,\ \Sigma a_3) \\
x_4 &= f(\Sigma l_{4_1},\ \Sigma d_4,\ \Sigma k_4,\ \Sigma u_4,\ \Sigma n_4,\ \Sigma o_4,\ \Sigma t_4,\ \Sigma a_4),
\end{aligned} \tag{6}$$

where Σ is summation, the l's are zero order factors (*i.e.*, specific to one variable only), l_{1_1} being, for instance, $\lambda_1 l_1 + \lambda_1' l_1' + \lambda_1'' l_1'' + \cdots$, specific to x_1; b, c, d, e, k, u are first order factors (*i.e.*, common to two variables only), Σb_1 being for instance, $\beta_1 b + \beta_1' b' + \beta_1'' b'' + \cdots$, with $b, b', b'', \cdots$ common to x_1 and x_2; m, n, o, t are second order factors (*i.e.*, common to three variable only), Σt_3 being, for instance, $\tau_3 t + \tau_3' t' + \tau_3'' t'' + \cdots$, with $t, t', t'', \cdots$ common to x_2, x_3 and x_4; Σa are third order factors (*i.e.*, common to all four variables, that is, general so far as these four variables are concerned). All factors of the same order or of different orders are here considered independent of each other, and so are those within each summation sign.

The multiple factor theory of Kelley's requires him to take a minimal number of factors of any order which is consistent with the observed correlations. Thurstone solves for the least number of general factors like a to lead to consistency. Thomson assumes a universe of factors from which those in equations (6) occur in all orders, these determined by the expectation of joint probability based upon the proportions which each ability is supposed to sample of the universe of factors. The present writer assumes in general factors of all orders, these determined by the setting of the situation in which the abilities are measured.

In our discussion we have contrasted multiple factor theories with the two factor theory. This distinction is not logical, of course, for Spearman's theory is a multiple factor theory of a sort, postulating $n + 1$ independent factors for n correlated variables, n of the factors being specific, the additional one, general. What we shall mean here and in the paper to follow, however, by multiple independent factor theories are those which postulate numerous important factors of orders greater than zero without the *requirement* of only one general factor.

B. Correlational Results Expected on the Assumption of Multiple Factors

By means of Garnett's formula [1] (5, p. 96), the correlations turn out to be for a pattern in which, for simplicity, one assumes b, c, d, etc.:

$$r_{12} = (\beta_1\beta_2 + \mu_1\mu_2 + o_1o_2 + \alpha_1\alpha_2)$$
$$r_{13} = (\gamma_1\gamma_3 + \mu_1\mu_3 + \nu_1\nu_3 + \alpha_1\alpha_3)$$
$$r_{14} = (\delta_1\delta_4 + \nu_1\nu_4 + o_1o_4 + \alpha_1\alpha_4)$$
$$r_{23} = (\epsilon_2\epsilon_3 + \mu_2\mu_3 + \tau_2\tau_3 + \alpha_2\alpha_3) \tag{7}$$
$$r_{24} = (\kappa_2\kappa_4 + o_2o_4 + \tau_2\tau_4 + \alpha_2\alpha_4)$$
$$r_{34} = (\nu_3\nu_4 + \upsilon_3\upsilon_4 + \tau_3\tau_4 + \alpha_3\alpha_4).$$

The observed magnitudes of each r will depend entirely upon the sum of the products of the weights of the factors common to the two correlated variables. The tetrad differences formed from these r's will take values, therefore, depending upon the values of the weights. The two-factorists have assumed that all weights are zero except the α's and have noted, as a consequence, that the tetrads vanish. But this is a very narrow view, for many other envisagements of the nature of the weights will also lead to vanishing tetrads. Note that the tetrad *equation* is

$$r_{12}r_{34} = r_{13}r_{24} = r_{14}r_{23} \tag{8}$$

[1] Garnett credits Bravais with this fundamental formula. It seems also to have been independently derived by Wright (36, p. 567), Kelley (11, p. 45) and Thurstone (34, p. 409).

and that substituting the terms in parentheses of (7) for their respective r's of (8) requires only that

$$(\beta_1\beta_2 + \cdots)(\nu_3\nu_4 + \cdots)$$
$$= (\gamma_1\gamma_3 + \cdots)(\kappa_2\kappa_4 + \cdots) \qquad (9),$$
$$= (\delta_1\delta_4 + \cdots)(\epsilon_2\epsilon_3 + \cdots)$$

in order that the tetrad differences vanish. It should be immediately apparent that the weights of the different factors in the different variables can take almost an infinite series of values producing very dissimilar r's and still lead to (9) and vanishing tetrads. In fact, the more factors at work, the more one would *expect* the product of each set of two parentheses to balance.

In this regard the work of Thomson and his associates is illuminating. Using playing cards and dice set up in accordance with his multiple factor theory, Thomson has shown that the greatest frequency of tetrads has the value zero (**32**, p. 244), and proved that 'zero is in every case the most probable value' of the tetrads (**32**, p. 253). In another paper he gave several further illustrations of the fact that, assuming all-or-none elements, the expected value of the tetrads is zero (**33**). Mackie followed these empirical demonstrations by a formal mathematical proof (**13**). In 1929 Mackie generalized Thomson's propositions and showed that even assuming variable elements, the mean value of the tetrads is zero (**14**, p. 27). Thus it is apparent that *the fact of mean value of tetrads being zero in any correlational problem serves no more to support the two factor theory than any multiple factor theory*. As Thomson says, "The difference between the Theory of Two Factors, and the theory that the variables are samples of a complex of factors, is not, that the former gives F (*i.e.*, the tetrad difference) = 0 while the latter gives a different value for F. Both give $F = 0$. The difference is that on the former theory *all* tetrad differences are *exactly* zero, while on the latter theory zero is only the most probable value, the actual theoretical value being distributed about zero." (**32**, p. 253.)

In other words, the members of (9) are expected in general to approximate equality, and whatever imbalance occurs leads to tetrads which take values around zero, but with decreasing frequency as one goes farther from zero. But even on a multiple factor theory, this scatter of tetrads will be sensibly affected by the *number* of factors postulated. As Thomson has shown empirically (**33**) and Mackie has shown mathematically (**14**, p. 37) the more factors there are at work the smaller the dispersion of the tetrads around zero.

To sum up, from multiple factor theories one would expect the 'true' tetrads to show an average value of zero. Many of the tetrads would thus satisfy very closely the tetrad difference criterion but some would disperse around zero. The more numerous the factors, the more tetrads would satisfy the criterion. The throwing out of the large tetrads would have no significance on the multiple factor theory, for it would mean simply that those tetrads left with approximately zero value would emanate from those variables whose factorial weights are in such proportions as to satisfy equations (9). This last point should be remembered in connection with the well-known tetrad-exclusion technique used by the two-factorists.[2]

C. Reversibility

Reasoning back from tetrads to factors, it must be obvious that the multiple-factorist has everything in his favor. The mean tetrad approaching zero would have little significance, since he would expect *that* on all patterns. A large dispersion of 'true' tetrads would indicate a high preponderance or heavy weight of low order factors. A small or zero dispersion would mean either the operation of many factors, or the presence of a few low order factors in such a happy combination as to satisfy condition (9). Ordinarily, however, *he would expect some variance of the true tetrads around zero.*

[2] Spearman has repeatedly stated that under the independent multiple factor theory, a large number of factors would lead to all individuals obtaining the same total score. This proposition may be tested by dice throws, and amounts to saying that if X = total faces on n dice, $\sigma_x^2 = 0$ when n is very large. But $\sigma_x^2 = n\sigma_d^2$, where σ_d^2 is the variance of one dice. Hence as n gets large σ_x^2 becomes large and Spearman's proposition is false.

In view of the fact (shown later) that the tetrad difference criterion is never satisfied if the values of the true r's are closely approximated, it would seem pedantic to enquire whether, if the tetrads did all vanish, the variables could always be expressed in terms of multiple factors. The question has academic importance only because Spearman has frequently stated that such a factoring could not be made. "Thus, the tests cannot possibly [where the criterion is satisfied] be so divided as to admit of any group factors (of appreciable size). Again, the tests cannot possibly be so divided as to have any general factor other than our g" (**23**, p. 113). "No one has even attempted to prove . . . that when the criterion is satisfied, then the division of the test-score is possible" (p. 151). On the contrary, several writers have shown that, whether the criterion is satisfied or not, if the intercorrelations do not exceed certain magnitudes, the variables can *always* be factored into 'group factors' *without the need of any general factors* (**30**, **31**, **3**). Furthermore, the addition of a few *independent* but general factors and high order factors would suffice to cover any case. In this connection, Garnett says, "It may be shown that n correlated q's [our x's], each of which is distributed according to the normal law with the same probable error, can always be expressed with $\frac{1}{2}n(n-1)$ degrees of freedom, in terms of n independent variables" (**5**, p. 100).

But Garnett has also shown that when these variables satisfy the criterion, they may be factored into the familiar two factor pattern. Spearman has made much of Garnett's proof, showing how variables caused by multiple factors but satisfying the criterion could always be split into two factors. But such a transformation, while a familiar mathematical device, leads to absurdities in interpretation. Such absurdities result from the attempts of the two-factorists to force their theory into agreement with multiple factors, and are represented by such statements as that by Spearman "the multitude of group factors without any manifest general factor, and the single general factor without any manifest group factors . . . can quite well *both exist together*" (**19**,

p. 323, italics his). "There is nothing to prevent *both* being true at the same time" (**22**, p. 559). "It appears that each of (the multiple factors) had really introduced a little bit out of the *g* together with a little bit out of the *s* . . ." (**18**, p. vii). Anyone who has set up artificial multiple factor patterns leading to vanishing tetrads cannot help seeing many difficulties in such an attempt to read a *g* into the patterns. At best, to force a meaningful *g* into multiple factors, Spearman must resort to a 'sub-theory' of *g*, representing the general factor as a rather vague amorphous abstraction. The absurdities into which the identification of *g* in multiple factors leads one, have been cleverly shown by Mackie (**12**).

When the tetrads *fail* to vanish, however, the two-factorist is in a bad way. This situation immediately throws his whole theory under suspicion, for the sole criterion which justifies even its consideration has not been satisfied. Such a situation, however, is meat for the multiple-factorist who *expects* in general that when the true correlations have been closely approximated some tetrads will not vanish. He has innumerable patterns to draw from which 'explain' the results. If he is parsimonious, he uses Kelley's iteration method (**11**, p. 79 ff.) or Thurstone's technique (**34**) to find the simplest patterns to choose from. If he believes in the bountifulness and complexity of nature, he looks for a multiplicity of factors.

IV. The Evidence
A. Methods of Analysis

We are going to consider some of the best studies on the intercorrelations between mental abilities to see whether two factors are adequate or whether multiple factors must be postulated. This will be done by calculating the tetrads in each problem and noting whether they differ from zero to a degree greater than the amount expected by sampling errors. This analysis may be made in either of two ways:

1. Calculation of All the Tetrads

We may calculate all of the $3C_4^n$ tetrads arising from the *n* variables in a given problem. These *observed* tetrads may

then be compared with those *expected* if they were all truly zero but took values by virtue of sampling errors only. This method of comparison has been succinctly described by Spearman as follows: "The most perfect comparison between the two, the observed tetrad differences and those to be expected from sampling errors alone, is obtained by making a complete frequency distribution of each of these two sets of values. A more summary comparison is got by seeing whether or not about half of the observed tetrad differences are greater and half less than their 'probable error' . . . . Most summary of all is to see whether or not the largest observed tetrad difference exceeds about five times the magnitude of their probable error" (**18**, p. **141**). It is plain here that truly zero tetrads affected only by errors of sampling are assumed to distribute themselves according to the normal law. In fact, in his *Abilities of Man*, Spearman plotted normal curves upon distributions of tetrad differences.[3] There is considerable doubt that the distribution of tetrads is normal, as Kelley (**11**, p. **14**) has pointed out.

We shall, however, use the same technique and assumptions in treating the data as are used by two-factorists (and with no greater validity, to be sure). We shall make the 'most perfect comparison,' namely, that between the distribution of observed tetrads and the normal distribution of tetrads expected on the two factor theory. If the two distributions fit at all closely we shall conclude with the two-factorists that the observed tetrads are truly zero tetrads dispersing because of sampling errors, and shall accede that a two factor determination is a tenable hypothesis. But if the observed tetrads show magnitudes greatly in excess of those expected by chance, we shall throw aside the theory as untenable. If multiple factors are at work, we shall expect, in general, besides the variance due to sampling errors an

[3] In his rejoinder on a criticism from Pearson, Spearman makes the surprising statement: "Even as a merely empirical and approximate fact, the normality forms no part of my theory" (**20**, p. **97**). In two factor analysis, the theoretical hand seems here not to know what the experimental and statistical hand is doing. It is to be noted that Holzinger, in his statistical resumé of the two factor theory, assumes normality to be so relevant a matter that he measures the goodness of fit of the observed tetrads with the normal curve by the (P, χ^2) test (**9**, p. **35**).

added variance resulting from some of the tetrads *really* being greater than zero. We are therefore interested in seeing whether the proportion of the total number of tetrads which have magnitudes larger than a given value, *t*, is equal to or greater than that expected by sampling errors. We shall graph the results as in Fig. 1. In this figure it is sup-
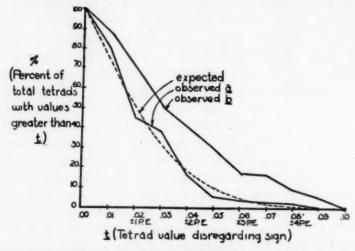

FIG. 1. Cumulative distributions of tetrad differences, the abscissa representing the absolute magnitudes of the tetrad differences (disregarding sign), the ordinate the proportion of the total tetrads under consideration having values greater than the magnitude shown on the abscissa. Both the 'expected' and 'observed *a*' curves represent the results if two-factors alone operate, the 'expected' being the theoretical cumulative 'normal' curve with a sigma calculated from 'formula 16A,' the 'observed *a*' being what one might expect of actually observed tetrads. The 'observed *b*' curve indicates what one would in general expect of actually observed tetrads if multiple group factors without a *g* were the determiners of abilities.

posed that the theoretical probable error of the tetrads due to sampling errors is ± .02. Thus, to illustrate, the theoretical curve, *expected* on the two factor theory, shows 100 percent of the tetrads greater than ± .00, 50 percent are greater than ± .02 (*i.e.* ± 1 P.E.), practically none are greater than ± .09 (*i.e.* ± 4.5 P.E.), in accordance with normality. Now if the observed tetrads show cumulative frequencies as indicated in *curve a* in Fig. 1, then it is quite apparent that the observed tetrads behave approximately as expected on the two factor theory and the theory is therefore

tenable. But if the observed tetrads appear as in *curve b*, then the tetrads are taking values much greater than those expected by chance errors, and multiple factors must be postulated. The considerations which led to plotting the cumulative curves of absolute values of the t's instead of the ordinary curve distributed about zero are (1) the lack of significance of zero as the center of gravity, a mean of zero being expected in general on all theories, and (2) the belief that the cumulative frequency greater than a given value, t, would have greater reliability than the limited frequency at a particular value.

The theoretical value of the P.E. of the tetrads will be determined by the well-known 'formula 16A' given by Spearman in his *Abilities* (p. xi). It is the square root of the mean of all the squared P.E.'s of all the tetrads taken individually (**26**). There has been some dispute as to the validity of the elementary formula of the individual tetrads upon which '16A' is based (**11**, pp. 13, 49; **35**), but we shall retain it since we wish to keep our technique as similar to that of the two-factorists as possible.

2. Calculation of a Crucial Sample of Tetrads: the T-Scheme

Since we are to consider several problems involving a large number of variables the writer has devised a scheme whereby, in such problems, only certain crucial tetrads need be calculated. If all the $3C_4^n$ tetrads vanish, these crucial tetrads will likewise vanish, and if the total number do not vanish, neither will the crucial ones. Thus, instead of 210, 378, and 630 total tetrads usually calculated for 8, 9, 10 variables respectively, the crucial tetrads calculated by the T-scheme are 18, 30, and 30 respectively.

This scheme makes use of the following convenient notation. For the four variables, x_1, x_2, x_3, and x_4 let T_{1234} stand for the *triplet-set* of tetrads shown in equation (5). Let T'_{1234}, T''_{1234}, and T'''_{1234} stand for the first, second, and third tetrad, respectively, of the triplet-set, namely, t_{1234}, t_{1243}, and t_{1342}. The number of primes thus indicates the tetrad in question, the absence of primes indicates that all three tetrads are under consideration. Now, if the tetrads of

T_{1234} vanish, then the six common factors, denoted by (12), (13), (14), (23), (24), and (34), may be considered to be the same general factor, a, as depicted in pattern (1), or independent group factors as depicted in system (6) satisfying conditions (9). Let us now introduce two more variables, x_5 and x_6. Here we have nine more common factors to deal with, namely, (15), (16), (25), (26), (35), (36), (45), (46) and (56). To test whether these common factors may be considered to be the same general factor, a, all that is necessary is to throw r_{15}, r_{16}, r_{25}, etc., into additional tetrads involving *some of the first six common factors*, and if the additional tetrads vanish, the new common factor may be considered to be the same general factor as indicated in T_{1234}. The additional tetrads may be set up in different ways, but the writer suggests the following: T_{1256} and T_{3456}. For six variables, then, the tetrads considered would be:

$$\left.\begin{array}{l} T_{1234} \text{ involving twice the common factors } (\textit{12}), (13), \\ \qquad (14), (23), (24), (\textit{34}) \\[4pt] T_{1256} \text{ involving twice the common factors } (\textit{12}), (15), \\ \qquad (16), (25), (26), (\textit{56}) \\[4pt] T_{3456} \text{ involving twice the common factors } (\textit{34}), (35), \\ \qquad (36), (45), (46), (\textit{56}) \end{array}\right\}. \quad (10)$$

In T_{1234} and T_{1256}, the common factor (12) has occurred in tetrads with ten other common factors; in T_{1234} and T_{3456}, the common factor (34) has also occurred with ten others; and in T_{1256} and T_{3456}, (56) has occurred with ten others. But (12) has occurred in tetrads also with (34) and (56), (34) with (12) and (56), and (56) with (12) and (34). Thus if the tetrads of the T's of system (10) vanish, then (12), (34), and (56), italicized in (10), may be considered to be the *same* common factor, and so may all the other factors with which they have been associated in the T's. But these are all of the factors involved in the problem, hence they may be all considered to be the same factor, namely a general factor.

With eight variables, the same principle is extended, the T's to be calculated being T_{1234}, T_{1256}, T_{1278}, T_{3456}, T_{3478}, T_{5678}. With n variables, the T's would be T_{1234}, T_{1256}, $\cdots$,

$T_{12(n-1)n}$, T_{3456}, $\cdots$, $T_{34(n-1)n}$, etc. With an odd number of variables, for instance, say $n = 7$, the T-scheme would call for T_{1234}, T_{1256}, T_{1267}, T_{3456}, T_{3467}, T_{4567}. (In actual practice, the writer has never used this T-scheme in problems involving less than eight variables, since here the number of tetrads of the T-scheme are so small as to give a rather uncertain cumulative curve of tetrads.)

After the tetrads of the T-scheme are calculated, they are then distributed in the cumulative curve, and examined to see if they may be considered chance dispersions from zero. The sampling error formula which has been used is the Spearman-Holzinger 'formula 16A' considered above. Since the tetrads of the T-scheme include all the r's, it would seem that the formula would be applicable. One point of difficulty, however, has been that some of the r's, such as r_{12}, r_{34}, r_{56}, etc., occur with greater frequency than others in the tetrads of the T-scheme. This would cast doubt upon 'formula 16A' giving the *exact* value for this distribution of tetrads. For the particular case of eight variables, the writer has corrected for the unequal frequency of different r's occurring in the T's of the ordinary T-scheme, *i.e.*,

$$T_{1234}, \; T_{1256}, \; T_{1278}, \; T_{3456}, \; T_{3478}, \; T_{5678},$$

by adding the additional T's:

$$T_{1357}, \; T_{1368}, \; T_{2457}, \; T_{2468}, \; T_{1458}, \; T_{1467}, \; T_{2358}, \; T_{2367}.$$

In the total of 42 tetrads here required, each r enters six times, so that '16A' would seem more exactly applicable. For problems involving more than eight variables the ordinary T-scheme as described has been used. To note whether in such problems the overweight of such coefficients as r_{12}, r_{34}, etc., greatly affects the distribution of tetrads, the rough technique of eliminating the T' from each triplet-set, then observing the distribution of the remaining tetrads, and, again, restoring the T' tetrads, then eliminating T'', observing the distribution of, now, the remaining ones, has been resorted to. Such correcting for overweighted r's has never, so far, changed the conclusions drawn from the full distribution of

ordinary *T*-scheme tetrads in any of the problems on which the writer has worked. If the full distribution of *T*-scheme tetrads has indicated that the tetrad criterion has not been satisfied (within the sampling error), neither have the part-distributions.

The ideal technique would require a new formula denoting the sampling error of just those tetrads involved in the *T*-scheme, but until this is forthcoming, '16A' must be used as an approximation. One simple justification for using '16A' for our sub-samples of tetrads is the none too satisfactory one that we are doing just what other experimenters have already done. For instance, it has even been the common practice for two-factorists to use '16A' for sub-samples of the total number of tetrads from which some of the *r*'s entering into the calculation of '16A' had been *excluded*. See for example, Spearman (**18**, p. 154, lower figure) and Stephenson (**27**, p. 181; **28**, p. 258, 260 ff.; **29**, p. 335 ff.). It is very doubtful, however, whether refined calculations of a probable error formula for different sub-samples of tetrads where *N* is the same *and very large* would give values significantly different from '16A' for the total group of tetrads, so we may consider this formula to render a safe approximative value.

In the ten tetrad analyses made in the following pages of this paper, the *T*-scheme was resorted to in only five cases, and in the remaining five analyses the full $3C_4^n$ tetrad differences were calculated in each case. It will be observed, however, that every analysis leads to just the same conclusion, namely, that the tetrad difference criterion is not satisfied.

B. Analyses of Ten Important Researches on Mental Abilities

Researches based upon a small number of subjects cannot shed light on the question as to whether the abilities of men are determined by two factors or by multiple. As Thomson has shown, when abilities are artificially made up ahead of time from multiple or complex group factors, such that the true tetrad differences are known to have a real dispersion around zero, having so few as even 60 artificial 'subjects' is insufficient to prove that the observed tetrads have a dispersion greater than that shown by the sampling error formula

(32). He says: "The real conclusion is that with 30 or 37 subjects the data as observed have such large probable errors that they are consistent with either theory, and prove neither (p. 244). . . . It seems that it is impossible with 60 cases to distinguish Two Factors from a complexity of group factors, and further that a very much larger number of subjects than 60 is necessary" (p. 247).

It seems obvious, therefore, that we must pass over as inconclusive most of the researches on the intercorrelations among abilities. If we seek about for researches involving more than 100 subjects the number of available studies shrinks to less than a score. The writer has, in fact, found ten satisfactory studies in which the number of subjects in each was greater than 100 and the individuals measured in at least four mental abilities. We shall make a tetrad analysis of these studies to see if the criterion is satisfied in each. With respect to the possible bias of choosing for consideration only those studies which obviously do *not* satisfy the criterion, leaving unconsidered many that do, the present writer has, in fact, leaned over backwards. He endeavored to select from the experiments of two-factorists themselves those estimated by them to constitute evidence for their theory. The *only* requirement made was that of large samples. Thus, we are to consider an experiment by Spearman, and two by his students (Davey and Stephenson). Another (by Bonser) was accepted by Spearman as support of the theory, and still another (by Kelley) was similarly interpreted by Holzinger. The remaining five were selected because of the large samples and excellent experimental controls employed. If there are experiments more crucial to factor analysis than these, the writer is not aware of them.

1. *Bonser's Study*

One of the first studies utilizing a large number of subjects is that of Bonser on 757 individuals, who were measured with respect to the following abilities: mathematical judgment, controlled association, literary appreciation, selective judgment, and spelling. There is no point in reprinting the

23

correlation coefficients here, since the reader may always
turn to the original sources for these data. The present
account is taken from that by Spearman (18, p. 147), who
considered Bonser's results a support of the two factor theory.
The writer has calculated *all of the tetrads* involved, and the
distribution of these compared with the 'expected' distribu-
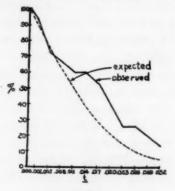tion (P.E. = .011) may be seen in Fig. 2. Instead of consti-

FIG. 2. Bonser.

tuting an unequivocal support of the two factor theory, the
observed distribution looks suspiciously as if there are too
many too large tetrads to be accounted for on sampling
grounds only. In proportion as we can accept the results
from the fifteen tetrads involved, we must lean towards a
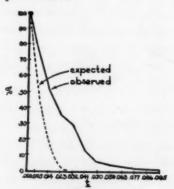multiple factor explanation.

FIG. 3. Spearman.

2. *Spearman's Study*

Spearman measured 2,599 members of the British Civil Service with respect to the following seven abilities: selective completions, inventive analogies, inventive completions, selective analogies, instructions, inventive passages, and selective passages. (The correlations are given in *Abilities*, p. 153.) I have calculated *all the 105 tetrads* involved, and the observed tetrads plotted with the 'expected' tetrads (P.E. = .007) are shown in Fig. 3. Here there is, of course, an obvious demand for multiple factors. Spearman has attempted to explain away the excessive divergence of the observed tetrads from zero by several group factors, but as to the consistency of his postulations we shall turn our attentions in the paper to follow.

3. *Davey's Study*

Working apparently under Spearman's direction, Davey tested 243 school children by eight oral tests (opposites, synonyms, classification, questions, completions, analogies, inference, likeliness) and six pictorial tests (classification, analogies, sequence, completion, questions, enumeration). I have calculated *the 63 tetrads of the T-scheme*. For the *r*'s see (4), Table A. The observed tetrads and the 'expected' ones
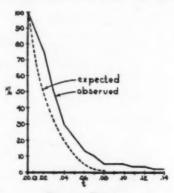

Fig. 4. Davey.

(P.E. = .019) are shown in Fig. 4. Clearly, again, there are too many too large tetrads for two factors to be adequate, and multiple factors are demanded. The group factors which

Davey necessarily had to introduce to explain away the excess divergence of the tetrads from zero were neither adequate nor consistent, as we shall later observe.

4. *Stephenson's Studies*

Using as subjects, 1,037 girls in elementary schools, Stephenson, research assistant to Spearman, obtained the intercorrelations between 16 verbal and non-verbal tests. The non-verbal tests were alphabet construction, code, fitting shapes, picture completion, form analogies, counting cubes, XO completion, overlapping shapes; the verbal were synonyms, sentence completion, classification, interchanged words, opposites, analogies, 'always has,' and directions. Stephenson examined first the non-verbal abilities for two factor determination, then the verbal abilities, and finally the two together. For the r's, see (27), Table III, (28), Table II, and (29), Table I (set I), and Table III (set II). I have calculated *the tetrads of the T-scheme* for each of these groupings, and the distributions of the observed tetrads are given in Fig. 5. The theoretical distribution of tetrads 'expected'
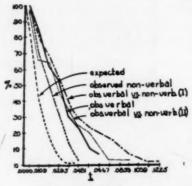

FIG. 5. Stephenson.

if two factors operated had a P.E. of the order .010 (Non-verbal .0095, verbal .0104, verbal vs. non-verbal .0105, .0100) as shown in the figure. There is not the faintest indication that the observed tetrads in any class fit the expected form on the theory of two factors. Furthermore, the painstaking efforts of Stephenson to warp these results

into the two factor theory were, as we shall later see, inadequate. Here, again, we observe a demand for multiple factors.

5. *Kelley's Study*

We shall examine the results from one of the experiments described by Kelley in his *Crossroads* (**11**), in particular the one which Holzinger later attempted to reconcile with the two factor theory (**8**). The subjects were 140 seventh grade children, the tests were of reading speed, reading power, arithmetic speed, arithmetic power, memory for words, memory for numbers, memory for meaningful symbols, and memory for meaningless symbols. The distribution of *the total 210 observed tetrads* and the 'expected' (P.E. = .029) is
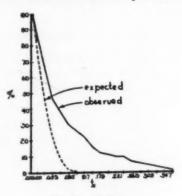
Fig. 6. Kelley.

given in Fig. 6. That the criterion is again not satisfied is evident, and that multiple factors are demanded is again manifest.

6. *Garrett's Study*

Working with memory functions, Garrett gave 158 college students eight tests, as follows: digit span visual, digit span auditory, paired associates visual, paired associates auditory, logical memory, digit-symbol, Turkish-English vocabulary, and code learning. For the *r*'s, see (**6**), Table II. The distribution of *the 42 tetrads of the special T-scheme* for eight variables is shown, together with the theoretical curve

(P.E. = .025) in Fig. 7. The inadequacy of two factors and
the demand for the multiple is again indicated.
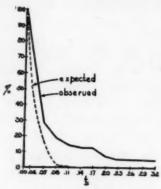
Fig. 7. Garrett.

7. Anastasi's Study

Dealing exclusively with the immediate memory of visually
presented material, and using 225 college students, Anastasi
secured measures of the following types: word-word p.a.
(paired associates), picture-number p.a., form-number p.a.,
color-word p.a., retained-members, and syllable-recognition.
For the tetrads, see (1), Table VII. The distribution of
all the 45 observed tetrads and the 'expected' distribution if
the two factors were at work (P.E. of tetrads by Kelley's
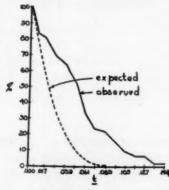
Fig. 8. Anastasi.

formula was of the order .02) are shown in Fig. 8. Obviously,
multiple factors are here again the determiners.

8. *Schneck's Study*

The investigation by Schneck dealt with verbal and numerical abilities. The subjects were 210 college students, who were given the following tests: vocabulary, opposites, analogies, sentence completion, disarranged sentences, and arithmetic reasoning, number completion, equation relations, and multiplication. For the *r*'s see (**17**), Table III. In
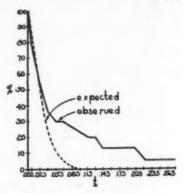
FIG. 9. Schneck.

Fig. 9 is given the distribution of *the observed tetrads of the T-scheme*, together with the distribution expected if two factors operated (P.E. = .023). Here again there are too many too large tetrads, precluding the possibility of the simple two factors, and requiring a more complex multiple factor postulation.

9. *The Minnesota Mechanical Abilities Study*

Turning now to the field of mechanical abilities, we consider the experiment of Paterson, Elliott, Anderson, Toops and Heidbreder. The subjects, 100 seventh and eighth grade boys, were given the following tests: card sorting, packing blocks, paper form board, Minnesota spatial relations, steadiness, Stenquist picture I, Minnesota assembly, and Otis intelligence. For the *r*'s, see (**15**), Table 35, and Table 50. The *42 observed tetrads* formed from the intercorrelations have been calculated according to the special *T*-scheme for eight variables, and give the distribution as shown in Fig. 10.

The theoretical distribution (P.E. = .032) is not approximated by the observed, there again being too many too large tetrads. Following the thought that the exclusion of Otis intelligence might show the remaining tetrads consistent with a general factor (not necessarily *g*), I have thrown out all tetrads involving Otis, but the situation is not improved, as
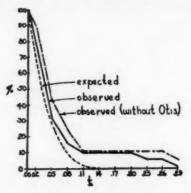
FIG. 10. Paterson, *et al.*

may be seen from Fig. 10. The monotonous fact again appears indicating that multiple factors are at work.

10. *The Studies of Hartshorne and May*

In the field of social performance and knowledge, Hartshorne and May secured measures of the following abilities: honesty, service, inhibition, persistence and moral knowledge. Approximately 200 children (Population Y) were used. The intercorrelations (7, appendix B) gave a total of only fifteen
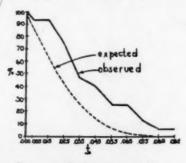
FIG. 11. Hartshorne and May.

tetrads, the theoretical P.E. being approximately .021. To the extent to which the distributions of these few tetrads in Fig. 11 have meaning, they indicate that the criterion is not satisfied and that multiple factors must be postulated for these social traits.

V. CONCLUSIONS

In the preceding ten important studies, representing the best efforts of psychologists in the measurement of the interrelation among human abilities, not a single study showed a distribution of observed tetrads which one would expect if simply the two factors suggested by Spearman were operative. In every case a significant proportion of the tetrads was too large, indicating that the tetrads truly dispersed about zero. This is a condition which *demands* that we postulate multiple factors as the determiners of abilities.

These conclusions are so strikingly opposed to those of the two-factorist that an explanation is necessary as to why our results show the tetrad criterion so universally not satisfied, whereas, two-factorists have claimed the reverse to be true. The reason is this, that the investigations made the most of by two-factorists have dealt with very small samples, but as Thomson has shown, even with samples as small as $N = 60$, the criterion is satisfied (within the expectations of sampling errors) both when the two factors operate and when the determination is by a multiplicity of independent factors. Thus the *crucial* test as to whether two factors or multiple factors are at work can be made only on large samples. We therefore selected for study the ten studies involving the largest numbers of cases and forthwith it appeared that the notion of the simple operation of the two factors is a myth.

How does the two-factorist reconcile such results to his theory? He attempts to do so by keeping his finger on his g and his s's, and then he introduces a few group factors or 'disturbers' which he claims are sufficient to explain the excess divergence of the tetrads from zero. This claim we shall consider in the paper which follows. But at this point, we should make this logical query. Why should he insist

on retaining his *g* and his *s*'s?   As we have seen in the reviewed evidence, there is not a shred which supports the view that such factors are at work.   If it had turned out that in the majority of such experiments the tetrads had vanished within their sampling errors, there occurring only a few exceptional experiments in which the tetrads had not so vanished, one would be willing to grant the presence of a few group factors sufficient to cover the exceptional cases. But when it actually turns out that apparently no single experiment exists in which, when the intercorrelations are accurately determined by using a large number of cases, the tetrads all tend to vanish, then it seems reasonable to reject the theory of two factors and to look for the multiple factor theory most consistent with existing psychological and biological findings.

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FORGETTING AND THE LAW OF DISUSE

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INTRODUCTION

Few, if any, of the experimental problems of psychology have been as actively investigated during the last fifteen years as have the fields of learning and memory. Particularly keen, on the side of theory, has been the interest in an explanation of fixation and elimination and in the general problem of the primary conditioning factors of learning. Interestingly enough, though some of the factual problems of forgetting have been vigorously studied, its theoretical aspects have been generally neglected. There are no warmly discussed theories of forgetting and there has been little research aimed directly at an explanation of the phenomenon. Where there has seemed to be need for a theory, the law of disuse has been briefly invoked and discussion has gone on to the facts of the curve of retention and to the influence of factors, mainly secondary, upon it.

THE IMPORTANCE OF FORGETTING

This neglect of the theoretical aspects of the problem is the more remarkable in view of the ubiquity of forgetting and of its general systematic importance. That forgetting pervades mental activity is so obvious a fact as often to go unremarked, and a few cases in point may be rehearsed briefly. In the process of learning itself, for example, forgetting is, paradoxically, an important factor. Were it not present, the curves for the acquisition of skill and for memorization would have forms to which we are now unaccustomed. Serial learning from the initial repetition to the final criterial fixation, is in most cases a matter of acquiring, partial or complete forgetting, reacquiring, and so on to the point

where fixation finally triumphs over loss. Forgetting is but one of several factors, of course, but it is undoubtedly a significant one.

In another sense forgetting is present in learning, in the alteration or loss which occurs in the parts, not only in those which are intrinsic and necessary, but also in those which are extrinsic and adventitious, of a total learning material such as a poem, a maze, or any other material, during the repetition of other parts. It is well known that as practice at any learning material goes on not only are 'wrong' acts and associations lost, and often those which are not wrong but only less effective than others, but a multitude of extrinsic associations and acts which are present earlier in the learning. As a result of these losses the total act is changed until it resembles only in its final outcome the act with which the learner started. This change is a result, to a significant extent, of the loss or forgetting of certain part-acts.

In other functions forgetting appears as an equally prominent factor. Perception, reasoning, emotional sensitivity, action are, in any general sense, a product of what the organism has forgotten as well as of what it has learned and retained. The generalization that mental activities characteristically alter with time rests on the fact that loss and its resultant changes, such, for example, as simplification, condensation, or incorrect substitution, have occurred, as well as on the fact of new acquisitions and related conditions. So pervasive are the phenomena of functional loss and its sequelæ that no theory of mental organization can disregard it, and that no systematic account of any of the major categories of psychology can be complete without at least implying, if not directly considering, its effects.[1]

Were forgetting limited only to the more conventional phenomena of the classical curves, it would be a significant

[1] The discussion has referred to forgetting and has made no mention of retention both because the emphasis is here on the former and because lack of retention (forgetting) is as important, theoretically, to the problem mentioned as is what has been retained. That is, mental activities are not only a function of whatever of past acquisitions has been retained, but also of the alterations which the activities have undergone through loss.

24

problem. In the light of its pervasiveness and generality, it becomes of fundamental theoretical importance, and a theory which can account adequately for its major characteristics will have implications which ramify widely into other major problems.

The Law of Disuse

The law of disuse, originally stated by Thorndike,[2] constitutes the only attempt at explanation which most writers make.[3] Thorndike's statement reads: "When a modifiable connection is *not* made between a situation and a response during a length of time, that connection's strength is decreased." It is the implication of this law, in whatever words different authors may couch it, that disuse, in and of itself, produces forgetting. Thus Gates [4] speaks of disuse as 'a passive state' in which trained mechanisms are left inactive, and of connections being weakened through disuse. He clearly regards disuse as the primary factor in forgetting. Woodworth,[5] likewise, holds to the potency of disuse and places the locus of its influence, as does Gates, squarely in the nervous system. In his introductory account of retention, Woodworth writes that "the machinery developed in the process of learning is subject to the wasting effects of time," and that an act is forgotten when its brain condition has disappeared through long disuse. Numerous cases of a similar usage may be found in the textbooks of both general and educational psychology. Writers seem to consider the law of disuse to be a statement of the major explanatory

[2] E. L. Thorndike, The psychology of learning, N. Y., Teachers College, 1914, p. 4. In The original nature of man, N. Y., Teachers College, 1913, p. 172, Thorndike gives another statement of the law: "To the situation, 'a modifiable connection not being made by him between a situation S and a response R, during a length of time T,' man responds originally, other things being equal, by a decrease in the strength of that connection."

[3] Explanations in terms of an hypothetical decrease in synaptic intimacy or of other neural changes customarily ascribe these changes to disuse. For the present discussion it does not matter whether disuse is employed alone and in general or in conjunction with some specific hypothesis of neural change.

[4] A. I. Gates, Psychology for students of education, N. Y., Macmillan, 1930, pp. 309–313.

[5] R. S. Woodworth, Psychology, N. Y., Holt and Co., 1929, p. 93.

condition of forgetting. Thus, curves of forgetting are referred to as curves which show the influence of disuse, certain methods of learning render the material less susceptible to disuse than do others, and the fact of forgetting itself is accounted for by the fact of disuse.

It may be urged that the law can be assumed to have significance only for a system of simple connections, since only on some such system can it be stated with any clearness. Then, since a simple bond hypothesis, involving a simple one-to-one neural basis, is of doubtful tenability, and since specification of the organismic locus of acquisitions is at present impossible, the law of disuse becomes at once out-moded on the ground that we are unable to state what is disused. There are several things which weigh against such a contention. If it were necessary to have definite knowledge of the organismic basis of learning before generalizations are formulated, virtually all of the generalizations thus far formu-lated would be invalid solely for lack of such knowledge. Probably few would hold that this is the case. So long as it is possible to measure and to control the appearance of a phenomenon, such as forgetting, by a control of its non-organismic experimental conditions, it should be possible to arrive at valid generalizations concerning it. Authors who are far from being subscribers to a bond hypothesis have invoked the law of disuse, and the present treatment of this law will assume that it extends, as well as to Thorndike's bond theory, to the disuse of any acquisition whatever and upon whatever theory. The considerations adduced apply to the law regardless of any particular theory of mental organiza-tion, and even though it is impossible to say what in the organism suffers disuse. If the material learned undergoes, for a given period, no observable or reportable use by the learner, we may validly refer to the period as one of disuse of that material.

It is the purpose of the following discussion to show (1) that forgetting and disuse are not correlated to the extent which the law assumes and that, therefore, the law, even if it were true in some cases, is not the general law which it

purports to be; (2) that in the situations in which forgetting and disuse are highly correlated, the disuse itself is not the important factor; and (3) that interpolated activities and changed stimulating conditions are the significant factors in the production of forgetting, factors which disuse, when it is correlated with functional loss, gives a chance to operate.

The Lack of Generality of the Law of Disuse

If there exist valid instances of the failure of disuse to be associated with any loss of an acquired function, or if valid losses occur in the absence of disuse, it must follow that the unit correlation necessary if disuse is to be employed as the explanatory law of forgetting is by no means present. In the face of a lower correlation disuse might be one contributing factor, other things being equal, but it could not be the sole law. One mode of testing the generality of the law is, therefore, to inquire if there exists any cases of the two kinds just mentioned.

In his critique of the laws of exercise and effect, Cason [6] has briefly attacked the law of disuse and has insisted that bonds are not necessarily weakened, and that they are sometimes strengthened, during disuse. In either case outside factors must be considered. Cason's arguments are cogent evidence against the law but, strangely enough, writers have continued to repeat it. Additional evidence, of a somewhat different kind, will be presented in the following paragraphs.

There may be found, in the experimental work on learning, certain clear exceptions to the generalization that disuse brings loss. One is the recovery of conditioned responses during a period of inactivity following experimental extinction. However the conditioned response may be regarded from a general theoretical point of view, it is an instance of learning, and its spontaneous recovery after extinction is the opposite of loss from disuse. The phenomena of reminiscence constitute a second exception.

[6] H. Cason, Criticisms of the laws of exercise and effect, *Psychol. Rev.*, 1924, 31, 397–417.

Ballard,[7] Williams,[8] and several other investigators have reported a higher percentage of recall one and two days after learning than immediately thereafter. The curves of retention in such cases rise during the first few days of the period of disuse and only then begin to fall. Reminiscence occurs only with some subjects, and with increasing maturity it tends to disappear, but over a relatively wide range it is present in some degree and its presence indicates a reversal of the expectation from the law of disuse.[9] The demonstration by Warner Brown [10] that successive recalls may elicit items not present in previous recalls and that, hence, retention is not measured, to its measurable limit with that method, by a single recall, is a limiting case of the law of disuse, since after a period of disuse items not previously recallable appear above the threshold of recall. All perseveration phenomena constitute, likewise, exceptions to the law.

We may inquire, next, if there are cases of forgetting which occur during use.[11] The experimental extinction of conditioned responses is one example of such forgetting and the closely related phenomena of negative adaptation are another. In both cases a given response drops out of the organism's pattern of response to a situation, is lost or forgotten in so far as that pattern and situation are concerned. Similarly in

[7] P. B. Ballard, Obliviscence and reminiscence, *Brit. J. Psychol., Monog. Sup.*, 1913, 1, No. 2, pp. 82.

[8] O. Williams, A study of the phenomenon of reminiscence, *J. Exper. Psychol.*, 1926, 9, 368–387.

[9] Should the reminiscence phenomenon be shown to be a result of intervening rehearsal, it would cease to have negative implications for the law of disuse. The conditions of the experiments, the generality of the phenomenon, its regular variation with age and material, and its regularity of duration argue against an explanation wholly in terms of rehearsal.

[10] W. Brown, To what extent is memory measured by a single recall?, *J. Exper. Psychol.*, 1923, 6, 377–382.

[11] It may be objected at once that to speak of forgetting from use is to confuse the meaning of the term forgetting, which has been employed traditionally to designate loss from disuse. By what criterion, however, can we differentiate between functional losses? It is hardly valid to employ the criterion of disuse versus use and to say that only those losses which occur during the former are to be classified as forgetting, since from the standpoint of measurable character the two kinds of losses are the same. The inadequacy of a use-disuse criterion will be more apparent still from the discussion of the second point of the paper.

point are the facts which Dunlap [12] has been reporting under the head of the beta hypothesis. Repetition breaks habits, eliminates them, causes them to be forgotten. Nailbiting, thumb-sucking, sexual perversions, and errors in typing are among the responses which are demonstrably lost by repetition. Related in implication are the phenomena of lapsed meaning with continued repetition or with continued fixation. [13] The phenomenon of refractory phase, in so far as it obtains in complex systems at least, represents a limiting case of forgetting. Repetition raises, in this case, a barrier against repetition of the same response, and this refractoriness appears to be the simplest case of forgetting from continued repetition.

In a somewhat intermediate class fall the large numbers of sudden losses of memories, such as appear, for example, in the hysterias. Such cases are well known and need only to be mentioned. The memories lost had not, for the most part, been in use immediately prior to their loss, but the disuse has never been, and can hardly be, alleged as the causative factor. Instead, active dissociating conditions are invoked. Such cases constitute a relatively large class of phenomena which lie outside of any reasonable explanation in terms of disuse.

The facts cited indicate that disuse does not always produce forgetting, that forgetting may sometimes be an accompaniment of use, and that there is a large class of abnormal phenomena lying wholly outside of the scope of disuse. It follows that disuse is not the general law which it purports to be. It might, in so far as the facts given show, be true in some or even in a majority of cases, but to be used accurately it would have to be modified and to have its limitations stated clearly.

THE INVALIDITY OF DISUSE AS AN EXPLANATORY FACTOR, EVEN WHEN PRESENT

The second point to be discussed, however, is that even in the situations in which forgetting and disuse are highly

[12] K. Dunlap, A revision of the fundamental law of habit formation, *Science*, 1928, **67**, 360-362; and Repetition in the breaking of habits, *Scient. Mo.*, 1930, **30**, 66-70.

[13] See, for example, V. J. Don & H. P. Weld, Lapse of meaning with visual fixation, *Amer. J. Psychol.*, 1924, **35**, 446-450.

correlated the disuse is not the important factor, and that the law is both logically and factually unsound. We shall consider first its logical aspects. Even were disuse and forgetting perfectly correlated, it would be fruitless to refer the forgetting to the disuse as such. Such reference is equivalent to the statement that the passage of time, in and of itself, produces loss, for disuse, literally interpreted, means only passivity during time. In scientific descriptions of nature time itself is not employed as a causative factor nor is passive decay with time ever found. In time iron, when unused, may rust, but oxidation, not time, is responsible. In time organisms grow old, but time enters only as a logical framework in which complex biochemical processes go their ways. In time all events occur, but to use time as an explanation would be to explain in terms so perfectly general as to be meaningless. As well might one use space or number. To say that mere disuse, time unfilled for the acquisitions in question, will account for forgetting is, even were the correlation perfect, to enunciate a proposition too general to be meaningful.[14] Time, in and of itself, does nothing. It contributes, rather, a logical framework in terms of which we can describe the sequence of observed events. Certain spans of it are necessary in order to give other and effective factors a chance to operate, and time, thus, figures largely in scientific description, but not as a factor in causal laws nor as itself active in any way. Nor does it appear descriptively in the same general way in which it is used in the statement of the law of disuse.[15]

[14] The objection may be urged that the law of disuse is not really intended to be an explanatory law and that as a descriptive law, instead, it is valid. There are two answers to this possible objection. Textbook writers and others write *as if* they considered disuse explanatory, and virtually all of the discussions of forgetting *seem* to assume the positive efficacy of mere disuse. If, however, we grant that the law is used purely as descriptive of the phenomenon itself and not of its necessary conditions, the same evidence tells against it. It is not the general description it purports to be; descriptive scientific laws are not given the logical form which the law of disuse has been given; and it is futile to describe in vaguely general terms when more exact description is possible. If the law of disuse described forgetting as some root of the elapsed time, for example, no objection could be offered to it as a descriptive formulation, but the law of disuse has never been stated in this way.

[15] It may be urged that writers who speak of 'the wasting effects of time' are using metaphor and that objections to such usage are but a product of too great a literalness. This can scarcely be taken seriously, for scientific laws are not usually stated in metaphor and a stated law deserves, if it merits anything, to be taken literally.

A closely related factual test is whether or not the law of disuse gives any real insight into the nature of the phenomenon of forgetting and whether it enables us to control the phenomenon experimentally. If the points already presented are valid, this question must be answered in the negative. If forgetting does not vary directly with disuse and if mere periods of disuse cannot logically be considered an important effective condition, the law can hardly carry us far toward prediction. It may seem as if the predictions possible from curves of forgetting contradict this, but it must be remembered that such curves are regularly the composite result of the operation of more than one condition, and prediction from them assumes that the operating conditions remain roughly the same. Here the real basis of prediction is the assumed constancy of conditions other than mere disuse.

EXPERIMENTAL EVIDENCE AGAINST THE LAW OF DISUSE AND FOR A POSITIVE EXPLANATION OF FORGETTING

It is one crucial test of the importance of sheer disuse to ask whether, with the period of disuse constant and with conditions of learning constant, forgetting can be made to vary by the introduction of variable conditions during the interval.[16] If these factors, rather than disuse, are the important conditions of forgetting, varying them should cause forgetting to vary. In this case two conclusions would be possible: (1) disuse is not a major condition of forgetting, since when it is constant the phenomenon appears and varies, and (2) these interpolated conditions constitute at least one major causative factor.

[16] When conditions of learning are constant, it cannot be said that there are antecedent factors which vary the conditions upon which disuse operates. When the length of the period of disuse is also a constant, if the disuse itself is the important factor, forgetting should be a constant, regardless of the conditions occurring during the interval. It cannot be said that these interpolated conditions only constitute limiting factors of the operation of the law, in the same sense that different degrees of learning of the original material might be limiting factors. Interpolated conditions have nothing directly to do with the original material. The latter is learned to a given degree and under given conditions before the period of disuse begins. Nor do the interpolated conditions have any relation to the period of disuse, as such, save that they appear during the period.

This crucial question has several times been put experimentally in experiments on retroactive inhibition and the answer, within the limits of the experimental situations used, is known. Experiments of this sort, however, if negative to disuse, do not prove that disuse, when given an opportunity to act, is utterly ineffective. They show only that other factors are also effective. The reverse experimental technique, whereby the conditions of original learning and of interpolation remain constant and the length of the period of disuse is varied, would be required to test the possible efficacy of disuse under optimum conditions for its appearance. Such an experiment has never been directly performed. It is virtually impossible to vary the period of disuse without varying also the number or kind, or both, of the conditions or events which fill the period. We can, however, from the available evidence make a very plausible inference regarding the results of such an experiment.

The experimental evidence pertinent to both of these questions is provided in the work on retroaction, and a review of the relevant facts will serve both to show the actual ineffectiveness of the law of disuse and to present the evidence for the third point of the paper. Jenkins and Dallenbach [17] have performed an important experiment which bears directly upon both questions. With constant conditions of learning, they compared the retention of syllables after equal intervals of sleeping and of waking. The variable here lies in the conditions of the interval, the waking intervals being much the more filled with interpolated events. This is true for periods of 1, 2, 4 and 8 hours, with the superiority of the sleep conditions becoming more pronounced as the interval increases in length. On the average, more than twice as many syllables are reproduced by both of their subjects after sleeping than after waking. Recall after sleep is sufficiently high to support the inference that, could an interval be rendered absolutely empty of events, a vacuum mentally, and the conditions of learning and of recall equated, there would be

[17] J. G. Jenkins & K. M. Dallenbach, Obliviscence during sleep and waking, *Amer. J. Psychol.*, 1924, 35, 605-612.

no forgetting. In their experiment, some time necessarily elapsed between learning and going to sleep and between being awakened and recalling; and sleep itself is not entirely empty of mental events. Consequently, if interpolated events produce forgetting, one would expect some forgetting to appear, as it did.

This high retention after sleep, the uniform superiority of recall after sleeping over recall after waking, and the fact that this superiority becomes greater as the length of the intervals compared increases, agree in pointing to the events which fill time as the effective factors in producing forgetting. Jenkins and Dallenbach validly conclude that their data indicate that forgetting is a matter of the active blocking of the old by the new, rather than of a passive decay.

Their results are important, also, for the second question raised above, whether disuse, if given an optimum occasion, can have any effect at all. The fact that a lengthening of the interval beyond two hours spent in sleeping produced no increase in forgetting, while a similar lengthening of the interval spent in waking produced increasingly greater loss suggests very strongly that the period of disuse itself is unimportant. Under the sleeping condition time interval varied without much variation in interpolation, yet forgetting failed to vary to any measurable degree beyond the two-hour interval.

These results have been partially corroborated by Dahl [18] with different methods and with more subjects. His interpretation is at variance with that offered by Jenkins and Dallenbach, but the important thing in the present context is the general agreement of his facts with theirs. Similar in implication is the evidence presented by Spight [19] that learning is more effectively completed after an interval, the major part of which has been spent in sleep, than after an equal interval of waking. In all of these experiments the interpolated materials have been the ordinary events of daily life which

[18] A. Dahl, Über den Einfluss des Schlafens auf das Wiedererkennen, *Psychol. Forsch.*, 1928, 11, 290–301.

[19] J. B. Spight, Day and night intervals and the distribution of practice, *J. Exper. Psychol.*, 1928, 11, 397–398.

bear, for the most part, no very close relation to the materials learned. These events are the major variables and the greater forgetting can validly be ascribed to them.

The conditions of the first test suggested have been fulfilled in several other experiments, the results of which need only to be mentioned briefly. They agree in showing that, with other conditions constant, variations in the interpolated events produce marked variations in forgetting. It is, for example, well established that the degree of retention of a given material is a function of the character of the interpolation.[20] In some of these experiments short periods of disuse are, with one interpolation, accompanied by very little forgetting and, with another interpolation, by almost complete forgetting. Likewise, when the degree of learning of the interpolated material[21] and the temporal point of interpolation[22] are varied, the degree of forgetting varies considerably. It would be out of place to summarize here the experimental researches on retroactive inhibition. It is sufficient to point out that they represent clean-cut cases of a wide variation in forgetting with a constant period of disuse.

It is the general result of the work on retroactive inhibition that, when the interval between the end of learning and the beginning of the measurement of retention is filled with some non-learning activity, involving as nearly complete rest from mental activity as is possible, retention is much greater than when the interval is filled with learning. The more nearly the

[20] See, *e.g.*, E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.*, 1920, **28**, no. 128, 1–57, and The 'similarity' factor in retroaction, *Amer. J. Psychol.*, 1927, **39**, 297–312; E. B. Skaggs, Further studies in retroactive inhibition, *Psychol. Monog.*, 1925, **34**, no. 161, 1–60; P. L. Whitely, The dependence of learning and recall upon prior intellectual activities, *J. Exper. Psychol.*, 1927, **10**, 489–508; L. M. Harden, A quantitative study of the similarity factor in retroactive inhibition, *J. Gen. Psychol.*, 1929, **2**, 421–430; N. Y. Cheng, Retroactive effect and degree of similarity, *J. Exper. Psychol.*, 1929, **12**, 444–449; J. A. McGeoch, The influence of four different interpolations upon retention, *J. Exper. Psychol.*, 1931, **14**, 400–413; and J. A. McGeoch & W. T. McDonald, Meaningful relation and retroactive inhibition, *Amer. J. Psychol.*, 1931, **43**, 579–588.

[21] J. A. McGeoch, The influence of degree of interpolated learning upon retroactive inhibition, To appear in *Amer. J. Psychol.*

[22] P. L. Whitely, The dependence of learning and recall upon prior intellectual activities, *J. Exper. Psychol.*, 1927, **10**, 489–508.

interval approaches complete rest, the better becomes the retention, and it is a plausible hypothesis that, with other conditions constant, a completely unfilled interval, were one attainable, would be accompanied by perfect retention.[23] It is evident from the facts that mere disuse is much less important than the law of disuse indicates. If the hypothesis just mentioned is valid, it follows that disuse is, as such, utterly unimportant. Its entire lack of direct significance is supported by the implications of the experiments on retention after sleep and after waking.

The considerations reviewed support, on the positive side, the hypothesis that retroactive inhibition, or interference from interpolated activities,[24] is one of the major necessary conditions of forgetting, and that without the presence of inhibiting interpolated events forgetting would not, in most cases, appear. The phrase 'in most cases' is inserted to leave room for the second condition, to be mentioned presently. Forgetting is, then, not a passive matter, but a result of an active interference from interpolated events.

That forgetting is an active blocking rather than a passive decay is strongly indicated, also, by the cases of almost complete reinstatement, under unusual conditions, of material not recalled for years and supposedly completely lost. Such cases are too well known to require description. They are afforded by delirium, hypnosis, free association, hypermnesia and the like. Even if we rule out many of the reported cases on the grounds of lack of adequate control and of inexpert observation, there are left a great many instances of well established reinstatement of past acquisitions a very long

[23] The crucial test of this would be afforded by a measurement of retention after a period of complete mental vacuity or absence of all mental events, all other conditions having remained constant. The experiments on retention after intervals of sleeping probably approach this as nearly as is possible experimentally. A measurement of retention after a period of complete anaesthesia, were it practical, would be very significant.

[24] The term retroactive inhibition is used to mean the fact that interpolated activities block or decrease retention. The words carry no implication regarding the nature of the phenomenon. The writer has argued elsewhere (On the term 'retroactive inhibition,' *Amer. J. Psychol.*, 1930, 42, 455-457) for the continued use of this term.

time after they are supposed to have been forgotten by the subject. A few only would be sufficient to cast doubt on the universality of the decay of acquisitions with disuse, and the substantial number of valid cases presents strong evidence that disuse does not necessarily bring loss but only, sometimes, factors which block reinstatement. It is useless to say that the special conditions, such as fever or strong emotion, 'heighten' memory. Had disuse brought decay there would have been no memory to heighten. It is more reasonable to suppose that the special conditions affect the conditions which had been blocking recall. How such conditions operate constitutes a fundamental problem, but the fact that it cannot now be solved in no way vitiates the conclusions drawn above. It is necessary first to analyze a phenomenon and to describe its conditions before the operation of the latter can become an independent problem.

The available evidence in support of the second factor, altered stimulating conditions, as one of the necessary conditions of forgetting is much less than is that in support of retroactive inhibition, but data from scattered sources and logical inference conspire to favor it. Some relevant stimulus is necessary for the elicitation of any response in the repertoire of an organism and in the absence of the requisite stimulus the response does not occur. It follows that forgetting, in the sense of functional inability or loss, may result from a lack of the proper eliciting stimulus, even when interpolated events have not been such as to bring the material below the threshold of recall at the time.

The absence of the necessary stimulus will occur as a result of change in the stimulating context of the individual. At least until learning has been carried far beyond the threshold, the learner is forming associations, not only intrinsic to the material which is being learned, but also between the parts of this material and the manifold features of the context or environment in which the learning is taking place. Two contexts must inevitably be present. One includes all of the stimulating conditions of the external environment; the other includes all intra-organic conditions. During time these

contexts alter and it is at least highly probable that such alteration may remove the necessary eliciting stimulus.

Large numbers of practical cases of forgetting seem to illustrate this. The missionary,[25] after being for some time in this country, loses his command of Chinese, but regains it, with almost no relearning, upon return to the stimulating environment in which he had learned and habitually used the language. One forgets the name of a person who appears unexpectedly, until some trick of speech, mannerism, or other aspect of the individual stimulates recall. The student fails to answer an examination question because it is phrased in a manner to which he is unaccustomed; perhaps the difference is only that synonyms for the familiar words have been used. In these and in many similar cases the material has not been lost from the subject's repertoire, but it cannot be reinstated when wanted; it has been lost functionally for a certain period.

Such cases of loss are related to retroactive inhibition, and the lack of the proper stimulus may often be a result of interpolated events, but it is at least possible that altered stimulating conditions may be in some cases a valid and necessary condition of forgetting, apart from retroaction. It is thinkable that a given material might suffer no decrement whatever from interpolated conditions, yet be unrecallable because of the lack of the proper eliciting stimulus. Studies such as Pan's [26] on the influence of context and of Yum's [27] on the law of assimilation offer such an interpretation experimental support. As research progresses, however, and as more is known about the mode in which interpolated conditions and altered contexts operate, it may be possible to bring both within a single hypothesis.

It is the hypothesis offered here that, in so far as the experimental facts now available show, the two necessary conditions for the appearance of forgetting in organically intact human

[25] Cited by H. A. Carr, Psychology, Longmans, Green, 1925, pp. 251–252.

[26] S. Pan, The influence of context upon learning and recall, *J. Exper. Psychol.*, 1926, 9, 468–491.

[27] K. S. Yum, An experimental test of the law of assimilation, *J. Exper. Psychol.*, 1931, 14, 68–82.

beings are interpolated activities and altered stimulating conditions.[28] To what degree these conditions operate is a function of at least two sets of factors: (1) the conditions and character of the interpolated activities and of the alterations in the environmental or eliciting stimuli; and (2) the materials upon which these factors operate. Under the first come such factors as the similarity between the original and the interpolated activities, the degree of learning and the point of interpolation of the latter, and the amount and character of the change in the stimulational context. Under the second come the factors of character of material, degree of learning, intelligence of the learner and the like. These constitute sub-conditions of forgetting, but are, according to this hypothesis, interpreted as limiting conditions upon the operation of the two primary conditions, but as being in themselves ineffective to produce forgetting.[29] Disuse, when it is correlated directly with forgetting, is interpreted to be the framework in which the effective factors operate. A period of disuse of the learned material is very often required to give the necessary conditions of forgetting time to act. It bears to forgetting the same logical relation that time bears to any of the processes of nature.

The well-known Freudian explanation of forgetting in terms of repression has been thus far omitted from consideration. To present adequate evidence of the interpretation is out of the question in the present space, but it is believed that all valid cases of forgetting from repression can

[28] These two factors are given as the reasons for the loss of habits by E. S. Robinson, Practical Psychology, N. Y., Macmillan, 1926, pp. 281–282. W. S. Hunter, Human behavior, Univ. of Chicago Press, 1928, p. 293, mentions these two factors, but lists disuse as coördinate with them. His discussion in Chapter 15 of The Foundations of Experimental Psychology, Worcester, Clark Univ. Press, 1929, implies the primary importance of interpolated activities. These factors are frequently described in discussions of forgetting, but not as being more fundamental than any of the other conditions affecting retention. In the present paper the writer has brought together the logical and factual evidence bearing against disuse and in favor of these as the major conditions of forgetting.

[29] The same general point of view has been briefly presented by the writer in Some phases of human forgetting, *Quart. J. Univ. N. Dakota*, 1928, **18**, 345–354; and Experimental studies of memory, in Readings in General Psychology, ed. by Robinson and Robinson, Chicago, Univ. of Chicago Press, 1929, 369–412.

be interpreted as cases of the operation of interpolated activities or of altered stimulating conditions with no wresting of the facts involved. Certainly the operation of affective factors may be brought within the scope of these two conditions.

THE NERVOUS SYSTEM AND FORGETTING

The statements of the preceding paragraphs have been made on the assumption of an intact nervous system. It is doubtful if any further assumptions about the place of the nervous system in forgetting are fruitful. The frequently made statement that forgetting is a matter of increased synaptic resistance, or of impaired connections in a network of neural arcs, is interesting speculation, but it does not advance our understanding of the phenomenon. No one has ever published experimental evidence that synaptic junctions decrease in intimacy, or in anything else, when one forgets. The notion is an inference from the psychological facts. The reasoning has, apparently, proceeded from the hypothetical description of learning as a matter of decreasing synaptic resistance. With time and disuse things learned are often lost. This is the fact. To account for it the synaptic resistance, supposedly lowered by learning, is assumed to be increased again as forgetting proceeds.

We know at least some things about forgetting and are able to arrange conditions experimentally so as to study its correlated factors. We know little about what happens in the nervous system when one forgets, and what evidence we have is against the notion of a resistance factor at the synapse.[30] It is more fruitful to work out our explanations at the level of experimental fact. When these facts are, some day, given their place in the neurophysiology and biochemistry of the organism, they will have been placed in a wider perspective, but it is doubtful if their specific significance will have been greatly altered.

There is, moreover, another point against the statement of

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[30] K. S. Lashley, The theory that synaptic resistance is reduced by the passage of the nerve impulse, *Psychol. Rev.*, 1924, 31, 369-375.

forgetting in terms of altered synaptic conductivity. Granting the truth of the hypothesis, we should still need to know, for the purpose of understanding and of experimental control, the conditions which affect such resistance, and no light can at present be cast on the problem save by inference from psychological facts. Some of the functional conditions of retention we know, and we can see how to discover others. The wisdom of foregoing the speculative delights of the nervous system for the world of experiment upon the phenomena of forgetting themselves and upon their measurable conditions is at least defensible.[31]

Summary

Evidence is presented for the conclusion that the law of disuse does not account for the pervasive and systematically important phenomena of forgetting.[32] (1) This law is not the general law which it purports to be, because of the lack of correlation between disuse and forgetting. (2) In situations in which forgetting and disuse are highly correlated, the disuse cannot be alleged to be the necessary condition of the forgetting. Ascription of effectiveness to time violates the usage of science and is logically meaningless. The notion of a passive decay has no analogue elsewhere in science and is, moreover, directly contradicted by the facts of the experiments on retroactive inhibition and of abnormal psychology. (3) The experimental work on retroaction shows, likewise, that forgetting varies with the interpolated conditions rather than with disuse and demonstrates empirically that disuse is relatively unimportant and probably entirely ineffective.

The positive hypothesis is advanced that the necessary conditions of forgetting are interpolated activities and altered stimulating conditions, and that the other factors of which

[31] The present account omits the forgetting which occurs from known organic causes, as in paresis, the alcoholic psychoses, and the like. Such cases are of psychological importance, but they are in a separate class from those discussed, all of which are non-organic in character to the extent that they can be experimented upon and controlled, at present, only by means of their related conditions.

[32] The usual and conventional phenomena of forgetting are the ones implied. Logically, forgetting is linked with other decrements, but the latter, while possibly explicable in a fashion coördinate with the explanation here offered for forgetting, are omitted from the discussion.

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forgetting is a function have the status of subconditions.[33] Disuse is important only in that it so often gives the primary conditions an opportunity to act. With further research it may be possible to bring the two major conditions mentioned under a single concept.[34]

[33] See, in this connection, the recent monograph by E. B. Skaggs, The major forms of inhibition in man, Chicago, Univ. of Chicago Press, 1931.

[34] The writer has discovered, after the manuscript of this paper was in final form, a paper by Marcel Foucault (Introduction à la psychologie de la perception. Expériences sur l'oubli ou sur l'inhibition régressive, *Rev. des Cours et Conférences*, 1913, 21, 444-454) in which he offers the hypothesis, on the basis of what was known in 1913 about retroactive inhibition, including data of his own from four subjects, and of certain anecdotal evidence, that forgetting is not the work of time but of intervening experience. Foucault does not elaborate either the hypothesis or the evidence, but his statement, in this too little known paper, is clearly an anticipation by eighteen years of the point of view expressed in the foregoing paper with respect to the importance of retroactive inhibition. He takes his problem, interestingly enough, from the same irregularity in the Ebbinghaus curve of forgetting from which the work of Jenkins and Dallenbach sets out, and suggests, as do they, that the irregularity is accounted for in terms of the proportion of the interval spent in rest and sleep.

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ON CHROMATIC AND ACHROMATIC COLORS

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The term 'color,' in the sense in which it is frequently used in psychology, includes both the chromatic colors (*bunte Farben*) and the achromatic or neutral colors (*tonfreie Farben*) —the whites, grays and blacks. Popular usage applies this term only to the chromatic colors, recognizing a fundamental qualitative difference between these colors and the members of the gray series. This difference is reflected in the construction of the psychologist's color pyramid, in which the grays are placed along the longitudinal axis, as well as in the use of a special term 'saturation' to denote dissimilarity to gray.

Among all the chromatic colors we recognize a common characteristic of saturation, or chromaticity, which distinguishes them from all the neutral colors.[1] From many points of view this two-fold division of the visual continuum is more fundamental, and more primitive, than the further division of the group of chromatic colors into a number of different *hues*.

According to Koffka (**31**, p. 267). the results of experiments on the development of color vision in infancy support the hypothesis that chromatic colors as a class are discriminated from neutral colors before the emergence of the capacity for hue discrimination.

In Ackermann's experiments on the chromatic threshold (**1**, p. 50 ff.), it was found that a very small amount of colored light was perceived as 'color' or as 'not-gray' before the observer could characterize it as having any definite hue; such a characterization became possible only when the quantity of colored light was considerably increased.

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[1] It is interesting to note that certain lower organisms spontaneously react in a characteristically different way to 'colored' as opposed to 'white' light—the so-called chromotropism (**39, 29**).

This primitive type of color perception is exemplified, in a most striking form in an abnormal case. This subject (who has recently been studied by Drs. W. D. Turner and H. R. DeSilva and the writer) is completely 'color-blind' in the sense that he cannot match or identify colors on the basis of hue. Nevertheless, he can quite readily distinguish a 'colored' object from objects which are white, gray or black. Thus, he seems to perceive all chromatic colors in the same way that Ackermann's subjects perceived threshold colors— he sees only a 'coloredness' which cannot be further qualified.

This antithesis between the 'colored' and the 'colorless' is the more remarkable when we reflect that it finds no counterpart in the domain of the physical stimuli. It is sometimes stated, in elementary discussions of color vision, that saturated colors are produced by homogeneous, or nearly homogeneous radiations, while unsaturated colors, and grays, result from the mixture of a large number of wave-lengths. Such a statement is highly misleading. By mixing light of a single wave-length with light of another single (complementary) wave-length, we can obtain a perfectly colorless mixture. On the other hand, with a much more complex stimulus, including, say, the wave-lengths from 790 to 660 mμ which make up one-third of the entire visible spectrum, we obtain a red sensation of maximal saturation. Thus the supposed correlation does not hold.

'White light' can be produced by a variety of complex radiations which are practically infinite in number. It so happens that 'white' is produced by one particular complex radiation which is of great biological significance, namely sunlight. As Troland has pointed out, "the function of chromatic vision in its natural operation is to detect deviations in the distribution of radiant energy over the spectrum from a certain standard distribution; viz., that of sunlight. The distribution in the solar spectrum is itself by no means uniform, yet it is the one which normally gives us the sensation which we call 'white.' This sensation not only indicates the null-point of the color process, but it is probably the visual quality which obtained for all operative stimuli prior to the

evolution of the chromatic system. This latter system must, then, have been designed to modify the character of the sensation in proportion as the spectral energy distribution of the stimulus *differed* from that of sunlight, and—so far as possible —in a characteristic manner for each specific and actually occurring deviation" (47, p. 33). Obviously, however, as our above example indicates, this design has been but imperfectly fulfilled. Thus chromaticity or 'coloredness' does not display an exact correlation with 'dissimilarity to the energy distribution of sunlight.' And we can hope to find an exact correlation only when we relate this property of sensation, not to the properties of the physical stimulus, but to the properties of physiological processes.

In our experience, the gray sensation forms a sort of 'dead level' within the continuum of colors. All our visual sensations possess the attribute of *brilliance*. (One can experience brilliance without chroma, but never chroma without brilliance.) But while an achromatic sensation is an undifferentiated brilliance,' a chromatic sensation is a 'differentiated brilliance.' A chromatic sensation is related to an achromatic, not by the *addition* of some new constituent, but by the possession of a new aspect, or mode of variation, which is superposed upon the fundamental brilliance characteristic.[2]

The principle of psychophysical parallelism would lead us to suppose that this relation between chromatic and achromatic sensations is represented by a corresponding relation between the cortical processes immediately underlying the two varieties of sensation. In other words, *the cortical process underlying chromatic sensation is of the same fundamental type as that underlying the gray sensation, but with some increased complexity, or higher order of differentiation.*[3] In what follows we shall suggest certain consequences of this idea.

Let it be emphasized that we are primarily concerned here not with the theory of the receptor mechanism, but with the theory of the central processes upon which visual experience

[2] In the acoustic domain, the relation between *tone* and *noise* is somewhat analogous.

[3] *Cf.* Troland and Goldstein, *infra.*

directly depends.   As is well known, Mrs. Ladd-Franklin has
developed the notion that the retinal apparatus subserving
chromatic vision is an evolutionary differentiation of a primi-
tive achromatic apparatus.   But as Troland has remarked,
"apparently the idea of the establishment of the luminosity
function . . . prior to the evolution of the chromatic system
does not enable us to decide whether this system involves a
plain *addition* to the luminosity mechanism or whether it
depends upon some sort of *differentiation* of the latter, as in the
Ladd-Franklin theory.   There can be no doubt that there is
a mere differentiation of the ultimate *cerebral* process because
of the form in which the chromatic mechanism manifests
itself in consciousness, but this does not necessarily imply
that the alteration of the retinal function is of the same sort"
(**47**, p. 35).

In so far as the retinal process is concerned, the various
theories may be divided from our point of view, into several
types.   (1) The first type is exemplified by the original Hering
theory (and the modified Hering theory as held by Tscher-
mak), which postulates a special 'white' process in the retina.
The isolated excitation of this process gives rise to an achro-
matic sensation; its excitation in conjunction with that of the
'color' processes produces a chromatic sensation.   The *brilli-
ance* of any sensation, chromatic or achromatic, depends solely
upon the degree of excitation of the *achromatic* process.   (2)
According to the later Hering theory (supported by Hille-
brand), not only the achromatic process, but the chromatic
processes contribute to the total brilliance.   In other words, a
'specific brilliance' pertains to the chromatic excitations.   (3)
On the Helmholtz theory, there is no special 'white' process,
but one sees white when the three chromatic responses, the
'red,' the 'green' and the 'blue' occur in equal amounts.[4]

[4] According to Hecht, white is a "central phenomenon"; "white occurs in the
brain when all three—red, green and blue—receiving fibers function in the retina"
(**24a**, p. 238).

As Hecht has emphasized, the white sensation can be produced by the binocular,
as well as by the monocular mixture of two complementary colors. This means that
a special achromatic process in the retina is not required in order to account for the
phenomena of color mixture, or for the unique status of white among the visual sensa-
tions. It may well be doubted that there are any other facts which oblige us to
postulate such a process.

Brilliance is, so to speak, wholly 'specific,' being derived from the summation of the values of the three chromatic processes. (4) Certain writers (Houstoun, 27; Weigert, 48) have proposed non-component theories of vision which interpret the relation between achromatic and chromatic sensations in terms of certain qualitative differences within a *single* type of complex retinal process. Brilliance and chroma represent different *aspects* of this total process. In other words, these authors postulate in the retina itself a process which has the same characteristics which we have postulated for the ultimate cerebral, or (to use Müller's term) the 'psychophysical' process. The retinal process which evokes a chromatic sensation is essentially like the process which gives rise to a pure achromatic sensation save for a certain difference in 'form.' [5]

It is not our purpose to prejudice this question of the nature of the retinal processes, and our view may well be compatible with any of these four types of theory. The fourth type has certain attractions, but the ideas of the authors cited have been very incompletely worked out, besides being very difficult to reconcile with the facts of nerve physiology.

Nor are we committed to a particular theory regarding the nature of optic nerve conduction. The following possibility is, however, interesting. If (and there is no convincing evidence to the contrary) different color qualities, as well as different intensities, can be mediated by a single optic nerve fiber, then since the frequency of the nervous impulses corresponds to intensity (2), one must apparently conceive of some sort of 'form-quality' which shall mediate chroma. Troland (46), who was perhaps the first to call attention to this point, has suggested that color-quality may be represented in the optic nerve process by a differentiation (group frequency) which is superposed upon the individual impulse frequency. More recently, Kohlrausch (33, p. 1491 ff.) has put forward the theory that quality is mediated by characteristic *changes* in the frequency of the optic nerve impulses.

[5] Since in the Ladd-Franklin theory the retinal substrates for 'whiteness' (brilliance) and for chromatic color are distinct (even though chemically related), this theory really belongs to our group 1 rather than to group 4.

If such a theory is correct, then the conception for which we are arguing would apply not merely to the psychophysical process but to the visual process in all its retro-retinal stages. Whether or not this be true, suffice it to emphasize that in the final stage at least, brilliance and chroma are represented as integral aspects, and not as additive components.

In the past, the 'theory of vision' has generally been regarded as coextensive with the theory of the retinal processes. Nevertheless, a great deal of evidence has accumulated which shows that the visual system contains a number of different functional levels, each with its more or less specific types of process, and that such over-simplified conceptions, as, *e.g.*, those of Helmholtz and Hering, are clearly inadequate.

In particular, there are facts which show that color vision,[6] as distinguished from mere brilliance vision, depends upon cerebral as well as retinal conditions. (This has been especially emphasized by Edridge-Green, **8**.) A number of cases have been reported in which persons who have suffered an injury to the visual cortex have become totally color-blind while still retaining the sensation of brilliance.[7] When a patient has undergone such a severe injury as to become temporarily blind, it is characteristic that he recovers sensitivity to light long before he can see color.[8] "The colors appear darker at first, and the patient makes color confusions, as in the case of the normal subject in reduced illumination" (**22**, p. 734).

To explain cortical color-blindness, certain authors (Wilbrand, **49**; Lenz, **37**; Edridge-Green, **8**) have postulated the existence of distinct centers for brilliance and for color. The fact that the brilliance function cannot be destroyed without a concomitant loss of the color function was explained on the assumption that the brilliance center is prior in the path of conduction.

[6] For the sake of convenience, we shall henceforth employ the word 'color' in the sense of 'chromatic color,' rather than in its more generalized sense.

[7] The color-blindness may be confined to half of the visual field (hemiachromatopsia), or to one or more isolated spots in the field (color scotomata).

[8] Halpern and Hoff (**23**) have described a case in which the subject was able to distinguish color only when the light intensity was unusually high.

In discussing this hypothesis, Goldstein says: "Whether the disturbance of color vision is to be ascribed a meaning in terms of localization, in the sense, *e.g.*, that certain layers [of the calcarine cortex] are significant for color sensation, and others for black-white sensation, as has been assumed in the past, may be left an open question. Such an assumption has little probability. The ever-recurring observation of an early reduction of color vision and of its late return during the restitution of processes, and thus of a lawful connection with black-white vision, suggests rather that the relation between color-vision and black-white vision is to be interpreted in terms of diversely differentiated activities of the same substrate, and that the disturbance of color vision is the first expression of the degeneration of the substrate" (**22**, p. 748 f.).

Moreover, color vision may be destroyed not only by a lesion, but, as Bordley and Cushing (**3**) [9] have shown, by a purely functional disorder, produced by the compression of a brain tumor, and returns when the compression is removed.

Thus the facts are most satisfactorily interpreted on the assumption that the visual process in the cortex is unitary in character, and that the loss of color vision is not due to the subtraction of *components* from this process, but to some kind of alteration in *form* within this integral excitation. There is an interesting parallel between this regression of visual sensation towards a more primitive type and the loss of 'differentiation' which is so frequently observed among other phenomena which accompany cerebral lesion (**36**).[10]

In certain cases of hemiachromatopsia, Fuchs (**19**, p. 526) found that a colored disk could be seen as colored over its entire surface if it were so presented that its center was on the boundary line between the normal and color-blind halves

[9] Cited after Piéron (**43**, p. 116).

[10] Gertz (**21**) has drawn an analogy between the tendency of colors, under a variety of conditions, to regress towards white, and the tendency, in the mechanical world, towards the degradation of mechanical energy into heat. An interesting example of this 'regression' has been observed by Dr. T. D. Cutsforth (**6**). In the course of his extensive studies on the imagery of the blind, Dr. Cutsforth has discovered numerous adventitiously blind persons who have lost all imagery for colors, although they can still summon up colorless visual images. Often the subject (who may be a habitual visualizer) is distinctly conscious of a progressive loss of chromatic imagery.

of the field of vision. This would indicate that the 'differentiation' of process within a given area is not a strictly local function, but is relative to the surrounding cortical field.

It should be noted that total color blindness [11] may result from pathological states not only of the brain, but of the optic paths (**38; 30**, pp. 191 ff., 264 ff.), and even of the retina (**30**, pp. 192 f., 236 ff.; **5**). This fact, which suggests an 'integral' as opposed to a 'component' theory of the optic nerve process, should not be lost sight of during the following discussion.

Besides these forms of acquired total color-blindness, there are other cases in which the condition is congenital. The most familiar form is the so-called 'cone-blindness,' in which the visual response is like that of the dark-adapted normal subject in reduced illumination. But several cases of total color blindness have been observed which do not conform to this description (**40, 10**). In these cases the brightest part of the spectrum is in the yellow region (as in normal daylight vision) and not in the blue-green (as in normal twilight vision and the vision of the cone-blind subject); thus the cones function, but they fail to mediate sensations of color.

In cases of this type the locus of the deficiency is uncertain. It is interesting to note, however, that the same type of colorblindness may result from purely central, functional causes— in hysteria (**30**, p. 307 ff.), and in hypnosis (**30**, p. 311 f.).

In normal *peripheral vision* we have a visual response of this same type. At moderate illuminations we are totally color-blind to stimuli at the extreme edge of the visual field. Although it has sometimes been supposed that this colorblindness could be explained by the fact that the rods greatly outnumber the cones at the periphery of the retina, this explanation is inadequate. For if we obtain the luminosity curve of the peripheral retina under this same illumination, we find it to be the type of curve which is characteristic of the cones, and not the curve, with Purkinje displacement, which

[11] Involving the complete visual field, or a more or less circumscribed area. The complete abolition of color vision is generally preceded by a period of red-green blindness.

represents the response of the retinal rods. It follows that in light-adapted peripheral vision we have cone vision without color perception (Von Kries, **34**).

But this color blindness is not absolute. If the intensity of the stimulus is very high, colors can be seen even at the extreme periphery   Color is sensed more readily, that is, at a lower stimulus intensity, the more saturated the stimulus. The sensitivity to color also increases with the area of the stimulus. Moreover, the sensitivity is greatest when the chromatic stimulus is presented on a background of equal brilliance (**26**).

Now, the weakness of the periphery is not confined to color perception, but extends to other functions, such as spacial localization and the perception of form. This fact may well lead us to suspect that the color weakness is not entirely due to retinal causes, but rests, at least in part, upon an inferiority of the corresponding cerebral apparatus.[12]

It is true that the deficiency of peripheral space- and form-perception is conventionally ascribed to conditions in the sense organ, and especially to the fact that the one-to-one relation between receptors and optic nerve fibers which exists in the fovea is not maintained in the periphery. I am inclined to believe, however, that such factors, while capable of explaining *inaccuracy* in peripheral perception, do not account for its peculiar *vagueness*, or low degree of organization.[13]

There is a suggestive analogy between peripheral vision and the defect of form perception (amblyopia) which, along with achromatopsia, is frequently found in cases of cortical lesion. As Koffka (**32**, p. 123) has remarked, form vision and color vision appear to be closely related; where form vision is disturbed, color vision tends also to be disturbed.

[12] That the cortical mechanisms of peripheral and foveal vision are not wholly identical in their mode of functioning is indicated by the fact that *contrast phenomena* (and phenomena of binocular summation) are more pronounced in the periphery than in the fovea.—Herwig (**25**) has argued for a central theory of peripheral color blindness, but on quite different grounds from those suggested here.

[13] As Exner (**11**) has pointed out, we may be able to perceive the parts of a pattern seen in peripheral vision as *distinct* from one another, and yet be unable to see how these parts are spacially interrelated.

It is noteworthy that in another region of the retina, namely *the border of the blind spot*, we find the same visual phenomena that are characteristic of the extreme periphery. If a chromatic stimulus is moved towards this region from an outlying retinal point, it gradually becomes colorless (**28, 42, 44, 24, 15, 7, 18**).[14] This change occurs, as Ebbecke (**7**) notes, in spite of the fact that the relative numbers of rods and cones around the optic disk are the same as in other retinal regions of the same eccentricity. He also reports that the loss of color vision goes *pari passu* with a weakening of *form perception*. One is strongly tempted to suspect that a central factor plays a role in the color deficiency as well as in the deficiency of form perception.

As we have remarked, the color blindness of the periphery is not absolute, but relative to the stimulus conditions.[15] In the periphery, there is a very large 'colorless interval' between the minimal intensity which gives rise to a sensation of colorless light and the minimal intensity which gives rise to a sensation of color. Other things being equal, this interval is smaller, the nearer we approach to the center of vision. But it does not vanish even in the fovea.[16] Thus the fovea itself exhibits 'color blindness' at very low light intensity. In this particular respect, then, the distinction between *fovea* and *periphery* is only a relative one.

In any retinal region, either central or peripheral, the chromatic threshold is relative to a number of different factors. It depends upon the physical quality and intensity of the stimulus. But it depends not only upon the nature of the stimulus proper, but upon the stimulation in the surrounding part of the retina. The color sensitivity is maximal when the surroundings have the same brilliance as the chromatic stimulus itself (**1, 26**); thus a color which is just

[14] The object suffers a change in hue along with its continual decrease in saturation, this change being similar to that which occurs in mid-peripheral vision (**28, 42, 7, 18**). It is also found that the border of the blind spot, like the periphery of the visual field, manifests enhanced *contrast* phenomena (**7**).

[15] This is true of the border of the blind spot also.

[16] Except in the case of very saturated red light, which, however, loses much of its saturation (**45**).

visible on a given background may be extinguished by making its surroundings either brighter or darker.[17]

Suppose we apply to any retinal region (foveal or peripheral) a stimulus which arouses a sensation of color. By altering the stimulus conditions in one way or another— *e.g.*, by changing the intensity of the stimulus proper or the intensity of its background—we can transform this chromatic sensation into an achromatic. Now, according to current visual theories, the distinction between chromatic and achromatic sensations is generally thought of as having a strictly *retinal* basis.

Consider, for example, the effect of lowered intensity of stimulus. Since there is a colorless interval even in the fovea, this effect cannot be attributed simply to the action of retinal rods; and, as we have seen, neither can the colorless interval in the light-adapted periphery be explained in this way. It has been explained by Hering and his followers on the assumption that the 'white' process in the retina has a lower threshold (with respect to intensity of stimulus) than the color processes.[18]

If Hering is right, then the process in the retina which is produced by an 'extinguished' chromatic stimulus should be identical in kind with that which would be produced by a pure white stimulus. But let us suggest an alternative theory. Namely, the extinction of color may not be a wholly *retinal* effect. The process which our stimulus produces in the retina may not have been transformed into a strictly achromatic process, and yet the remaining quantity of chromatic process may have been insufficient to produce, within the psycho-physical zone of the cortex, the requisite degree of 'differentiation' which is necessary to the perception of color.

This hypothesis is suggested by the following consideration. Instead of lowering the intensity of our stimulus, we can render it achromatic by the addition of white light. In this case we obtain a colorless sensation even though our

[17] Within limits, the color may also be extinguished by diminishing the area of the stimulus, or the time of exposure.

[18] This phenomenon has always presented a difficulty for the Helmholtz theory (*cf.* **45**).

chromatic stimulus is well above the threshold of the retinal color processes. Thus Hering's conception is over-simplified. The chromatic threshold is dependent not merely on the absolute amount of chromatic stimulus, but on the *ratio* between this amount and that of the simultaneous 'white' stimulus. And, as Von Kries (**35**, p. 272) has remarked, it is difficult to conceive of a sense-organ threshold which shall be governed by such a law. It seems more likely that the threshold is centrally determined.[19]

Another fact points in the same direction, and this is the dependence of the threshold upon the brilliance difference with respect to the background. This effect is doubtless allied to the familiar phenomena of *contrast*. (The perceptibility of an 'objective' color, like that of a contrast color, is favored by uniformity of brilliance within the field; thus a similar law obtains in the two cases.) But, according to the researches of Brückner (**4**) and Müller (**41**, p. 56 ff.), the physiological process underlying contrast occurs in the *cortex* (although at a lower level than the 'psychophysical zone' or region with which experience is directly correlated). This makes it appear likely that the chromatic threshold is conditioned by the cortical field, and not simply by the state of affairs in the retina.[20]

Leaving aside these considerations, we may cite direct evidence in favor of the view that a colorless sensation does not necessarily imply an achromatic retinal process. If the eye is fatigued with strong colored light, its brilliance sensitivity is reduced, and the reduction is greater for stimuli of the same hue as the fatiguing stimulus than for stimuli of different hue. This 'selective fatigue' is especially pronounced when the fatiguing stimulus is red (**12**). Suppose we fatigue a given retinal region with strong red light, and then test the effect of the fatigue on equally brilliant red and green stimuli which are *below the chromatic threshold*. There are two possibilities. Our subliminal stimuli may evoke per-

[19] This is not to say that the ratio between the *retinal* processes is necessarily unaffected by diminution of intensity.

[20] The experiments of Gelb and Granit (**20**) point to a similar conclusion.

fectly achromatic, and identical, processes in the retina. In this case there could obviously be no selective fatigue. Or the corresponding processes may still retain some trace of chromatic character, and hence be different. In this case a selective fatigue is possible. The experiment in question has been carried out by Engelking and Poos (9), using subliminal red and green stimuli of very small area which were presented within the fovea. After fatigue with red light, the 'red' stimulus, originally equated in brilliance with the 'green,' became definitely less brilliant. It follows that these subliminal stimuli must have aroused chromatic retinal processes, and that their subjective achromaticity was due to an absence of response at a higher level.

It has been shown by Fernald (13, 14) that a chromatic stimulus which, being viewed peripherally, appears colorless, may nevertheless be followed by an *after-image* of the complementary color. The extinction of color in this case must have taken place above the level of the after-image processes; and negative after-images, according to evidence cited by Müller (41, p. 246 ff.) are of nervous and not retinal origin. Moreover, Ferree and Rand (17) found that *chromatic contrast* could be produced by the action of a stimulus which was presented at such low intensity as to be below the chromatic threshold. This stimulus must have evoked a chromatic process in the retina, and the chromatic effect must have been transmitted as far as the 'contrast zone' of the cortex. The absence of perceived color must be ascribed to an insensitivity at a still higher level of the visual system.

These same authors have performed a series of interesting experiments on the inhibition of color which accompanies the addition of white sensation (16, 17). This white was added, not in the form of objective white light, but in the form of a white after-image, obtained by pre-exposing the eye to a field of black. They have found that this addition, while it may prevent the stimulus from arousing a *sensation* of color, "does not lessen its power to arouse after-images" (17, p. 208).[21]

[21] The papers cited give only a brief account of these experiments; apparently a complete report was never published.

As they say, "this strongly indicates that the action of brightness [achromatic process] upon color takes place at some physiological level posterior to the seat of the positive and negative color processes . . ." (p. 208 f.).

It must be remarked, however, that this conclusion may not be true for *all* actions of achromatic processes upon chromatic. In the case of the achromatic after-image, it may be that the confluence of the two processes occurs only at a central station of the visual system, whereas, when physical 'white' light is added to colored light, the two kinds of process coexist in the *retina*. Müller (**41**, p. 436 ff.) has maintained that there is a special 'antichromatic influence of white' which is peculiar to *retinal* mixture, as opposed to mixture at a higher level of the system. At any rate, we must still regard it as an open question whether the antichromatic effect of 'objective' white light is purely central or partly retinal. Further study of this problem is greatly to be desired.

We conclude then that a stimulus may produce a chromatic process in the retina, and yet fail to produce a chromatic process in the psychophysical zone. This may occur even though a chromatic effect is transmitted along the optic paths to the cortex. Thus the retinal conditions are inadequate, in the last analysis, to determine whether a given stimulus shall be sensed as colored or colorless; unless the appropriate central conditions are present, we see an 'undifferentiated brilliance,' *i.e.*, a white.

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THE ORGANISMIC HYPOTHESIS AND DIFFER-
ENTIATION OF BEHAVIOR

III. THE DIFFERENTIATION OF HUMAN BEHAVIOR

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Upon canvassing the contributions to the organismic con-
ception by the various fields of scientific investigation the
following data are found available: The cytologists have
contributed the view that the cell is not a separate independent
unit but is a locus of activities in protoplasm. The his-
tologists, working with regenerating materials, find that these
tissues pass through a generalized or syncytial and coenocytic
stage out of which structural and functional differentiations
appear. Experimental zoölogists are able to alter the polarity
of tissues and force new gradients upon them. Also in animal
forms higher up the phyletic scale they have traced the
appearance of behavior patterns and correlated successive
increments of neural maturation with these patterns. More-
over they have demonstrated that the reflex or behavior
segment is a comparatively late differentiation in ontogeny.
The embryologists have experimentally modified embryos in
the early gastrular stages and have studied the effects of trans-
planting limb buds on the direction of growth in nerves.
They have observed that the behavior of the human fetus
is of a massive type involving the entire organism and that
the younger the fetus, the greater is the generalized irradiation
of activities. The experimental neurologists have extirpated
brain masses in rats and monkeys without destroying adaptive
performance. The work in electro-physiology illustrates how
the organism and its tissues actually contribute to behavior.
Researches with premature and newborn infants extend the
findings on fetal materials and indicate that there is a
commingling of mass activity and specific movements.

26

These studies indicate that there is an increasing acceptance all along the line in biological science of the organismic interpretation. But all this, so far as the investigation of human behavior is concerned, is only a start. In spite of the convincing array of morphological data supporting the organismic hypothesis, it is not clear exactly how the process of individuation of *behavior* takes place during the uterine and postnatal periods of life. On the behavior side the organismic hypothesis waits for a body of experimental results from investigations in these two fields comparable to that on the morphological side. The problem of the differentiation of behavior is analogous to the problem of the differentiation of protoplasm or of the nervous system; yet until psychologists specifically show what are actually the processes whereby differentiation of human behavior proceeds, the relation will remain that of mere analogy. The relation needs to be directly established by experimental evidence if the organismic hypothesis is to hold on the behavior side. If this is true, then a broader and more concerted experimental attack on the problem of differentiation of patterns during early life is necessary.

It was suggested at the end of the second article of this series that mass activity is probably the earliest activity of the uterine organism, that segmental movements and specific patterns are aspects of it which become partly specialized in the uterine environment, and that both individuated and massive features are present in the behavior of the newborn infant. If these observations are true, then they possess implications for an experimental approach to the problem of pattern differentiation. Experimentation in early human behavior may profitably concern itself (1) with establishing the temporal order of differentiations of behavior from the early mass stages, and (2) with the nature of the transformation into their specific, more finely graded, and better patterned aspects. For example, this might be attempted with postures. The problem would be to determine their temporal appearance in the uterine and extra-uterine periods and their differentiation throughout the first years. It is

significant that some of these early postural patterns closely resemble those found by Magnus (**8**) in decerebrate animals. They undergo profound transformations during the first year of infancy. They are important because a careful and minute study of their stages of differentiation will throw much light on the problem of walking.

A type of experimentation which will illustrate the differentiation of a specific pattern from the generalized massive response may be proposed for further studies on grasping, smiling, and eating. The final stages of the grasping pattern have been studied by Halverson (**4**). However, the early aspects of the differentiation have not yet been worked out. The primary differentiation appears, not in the hand but in the shoulder and upper arm. It is in these segments that differentiation from the total body pattern starts. The differentiation proceeds to the elbow, forearm, wrist, hand and digits. Coghill's (**2**) observations on Amblystoma demonstrate this order for that form and some preliminary photographic observations on infants indicate a similar progression of differentiation down the limb culminating in grasping.

Smiling illustrates a differentiation from mass activity in the direction of specificity. Washburn (**9**) has studied this reaction beginning with infants at four months. However, smiling is observed as a response to internal stimuli as early as the first week. It is not a single specific pattern, but is observed as an aspect of massive generalized reactions. The specific aspect is a final differentiation which appears without the massive components and is evoked by external objects and by other individuals.

Another illustration of the principle of behavior differentiation is the feeding response which an infant makes when a spoon or a cup is employed. The earliest response is purely passive. With the development of vision a generalized mass movement of mouth, head, shoulders, trunk, arms and legs may be observed. As differentiation proceeds mass movements drop out and appropriate specific mouth movements occur.

Investigations of early social behavior in infancy might
employ a similar approach. The problem here, of course, is
quite complex, but the principle of starting with mass activity
and establishing temporal norms for the appearance of social
patterns and then intensively studying these patterns sepa-
rately in their genetic development is as feasible in social
behavior as in any other type of behavior. For example, the
first stages of language will lend themselves to such a pro-
cedure. It has been found with newborn infants that crying
is a component of mass activity. The first differentiation of
crying appears to be the hunger cry, a distinct variation of
general crying. An intensive study of the appearance and
transformations of the hunger cry, a minute analysis of its
vocal factors as it changes from week to week, and the
stimulating effects it exerts on the nurse, parents, members of
the household, would throw considerable light on early
speech as well as on the genesis of social behavior in infancy.

Studies of the type suggested above have been begun in the
Infant Laboratory of the Iowa Child Welfare Research
Station. The work has progressed far enough to warrant a
tentative formulation of a theory of differentiation of infant
behavior. Differentiation at birth has arrived at a stage
wherein it proceeds in several directions. For instance,
motion pictures of young infants seem to indicate that the
reaching and grasping pattern during the postnatal months
proceeds outward from the trunk down the arm from the
proximal to the distal segments. Thus grasping is the final
stage of a process of differentiation which begins in the
shoulder and upper arm. Likewise normal plantar traction is
the final aspect of a process of differentiation that first appears
in the hips and progresses distally in the leg. Coghill (2)
has established this principle for the individuation of both
structures and movements of the limbs of Amblystoma.

Moreover, film studies in infants from month to month
suggest the probability that differentiation may also occur
in an anterior direction from the trunk. On the neural side
this has been observed by Cowdry (3) in the chick. He
reports that the earliest neurofibrils to appear are formed in

the hind-brain in stages of 15 somites and on. Later they appear in the extreme anterior end of the mid-brain in stages of 18 somites and on. Likewise in the early stages of human development differentiation anteriorly is indicated by the fact that in the brain of the seven month fetus (7) there is little myelinization ahead of the mid-brain. It is practically certain, therefore, that anterior differentiation is already under way at birth. It is also known that myelinization in the cortex is not complete until many months after birth. The differentiation of facial expression, the finer development of vision, audition, and speech indicate that the process continues in an anterior direction on the behavior side during the postnatal period.

In the trunk itself studies of anti-gravity patterns reveal another direction of differentiation. The pattern originates in the cervical region and progresses through the thoracic segments until the lumbar-sacral muscle groups are involved. When an infant is placed on its back, motion pictures show that the attempt to achieve the sitting posture begins in the cervical region. When placed on its stomach the anti-gravity aspect likewise first appears in the cervical region. Motion pictures of infants held suspended in an inverted posture indicate that opisthotonus during the first months is confined largely to the cervical segments. Later it extends to the lumbar region. Forward bending in the suspended inverted position rarely occurs during the first half year of life. It is characteristic of the second half year. In this connection it is of interest to note that Langworthy's (5, 6) work on the spinal cord of kittens, pouch-young opossums, and the human fetus (7) has established the law that maturation of spinal cord tracts begins in the cervical cord and proceeds caudad. Coghill (2) has found the same to hold in the spinal cord of Amblystoma.
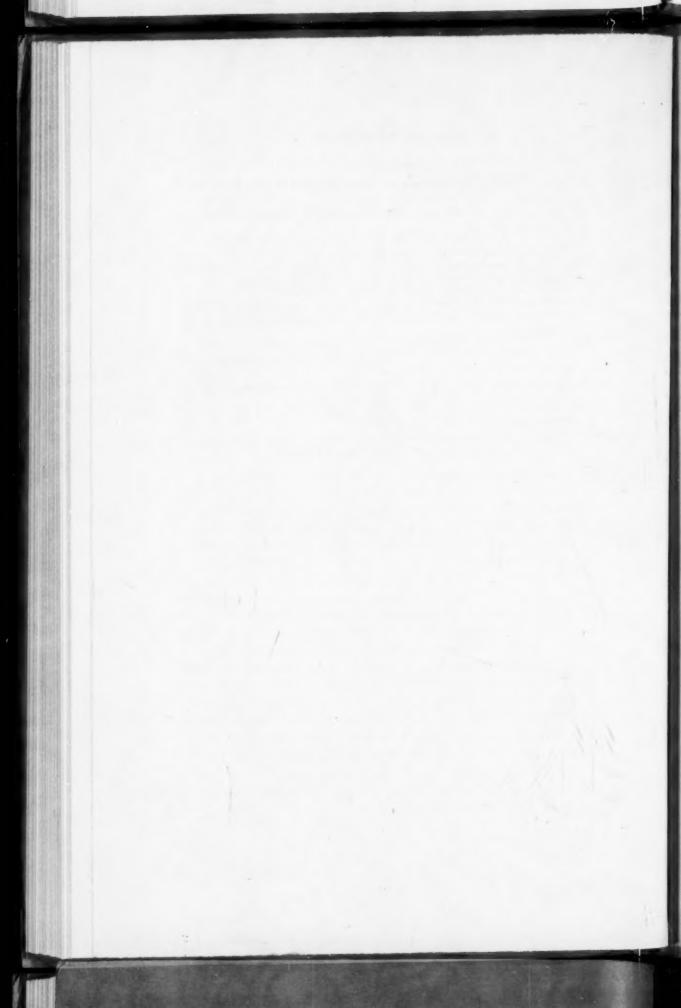
In a word, the theory of development of behavior sketched here implies that differentiation will be found to proceed radially in the human organism from the neck and cervical trunk region generally in the same way that the maturation of the central nervous system has been found to proceed

forward and back from the brain stem and cervical region. The writer is fully aware that this view of behavior differentiation has little more to its credit than the status of an hypothesis, but it appeals to him as a more helpful theory than the older reflex view so far as the activities of babies are concerned. A major problem in the study of human reactions is to investigate the actual nature of processes involved in pattern differentiation, and this hypothesis has afforded him a plan with which to attack experimentally the bewildering maze of responses from birth throughout the first year of life. After all, the immediate purpose of an hypothesis is to throw up problems and guide experimentation toward their solution.

In conclusion, the position set forth in this series of papers is briefly stated. The organismic hypothesis on the structural side assumes that the organism differentiates into cells, tissues, organs, and structures, all the time maintaining its integrity. The various zoölogical sciences afford a large and convincing body of evidence for the hypothesis. Turning to the behavior side of the problem, patterns differentiate from a primitive general matrix of behavior called mass activity. Mass activity is at its maximum during the first fetal months, and during uterine existence the differentiation of activity into patterns is under way. After birth the processes of differentiation proceed with increasing degrees of specificity, definiteness, and preciseness of pattern until they attain the degree of maturity which we recognize in the adaptive behavior of the organism. However, the fact should be emphasized that experimental evidence for an organismic view from studies on human behavior are only beginning to accumulate. The organismic theory of human behavior needs the clarification which can come from detailed and separate investigations of the kind suggested. The crucial point of attack is an experimental analysis of the processes of pattern differentiation during the first year of life. A radiate theory of pattern differentiation is suggested as a tentative guide to this type of analysis.

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